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NEW CONSTRUCTIONS IN GRAPHICAL STATICS.

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VII.

THE ARCH OF MASONRY.

Arches of stone and brick have joints which are stiff up to a certain limit beyond which they are unstable. The loading and shape of the arch must be so adjusted to each other that this limit shall not be exceeded. This will appear in the course of the ensuing discussion.

Let us take for discussion the brick arch erected by Brunel near Maidenhead England, to serve as a railway viaduct.

It is in the form of an elliptic ring, as represented in Fig. 14, having a span of 128 ft. with a rise of 24½ feet. The thickness of the ring at the crown is 5¼ ft., while at the pier the horizontal thickness is 7 ft. 2 inches.

Divide the span into an even number of equal parts of the type bb, and with a radius of half the span describe the semicircle gg. Let ba=24¼ ft. be the rise of the intrados, and from any convenient point on the line bb as b_5 draw lines to a and g. These lines will enable us to find the ordinates ba of the ellipse of the intrados from the ordinates bg of the circle, by decreasing the latter in the ratio of bg to ba. For example, draw a horizontal through g_3 cutting b_5g at i_3, then a vertical through i_3, cutting b_5a at j_3, then will a horizontal through j_3 cut off a_3b_3 the ordinate of the ellipse corresponding to b_3g_3 in the circle, as appears from known properties of the ellipse.

Similarly let bq=64 ft. + 7 ft. 2 in., and with bq as radius describe a semicircle. Let bd=24¼ ft. + 5¼ ft. be the rise of the extrados, and from any convenient point on bb, as b_5' draw lines to d and q. These will enable us to find the ordinates bd of the ellipse of the extrados, from those of the circle, by decreasing the latter in the ratio of bq to bd. By this means, as many points as may be desired, can be found upon the intrados and extrados; and these curves may then be drawn with a curved ruler. We can use the arch ring so obtained for our construction, or multiply the ordinates by any convenient number, in case the arch is too flat for convenient work. Indeed we can use the semicircular ring itself if desirable. We shall in this construction employ the arch ring ad which has just been obtained.

We shall suppose that the material of the surcharge between the extrados and a horizontal line tangent at d causes by its weight a vertical pressure upon the arch. That this assumption is nearly correct in case this part of the masonry is made in the usual manner, cannot well be

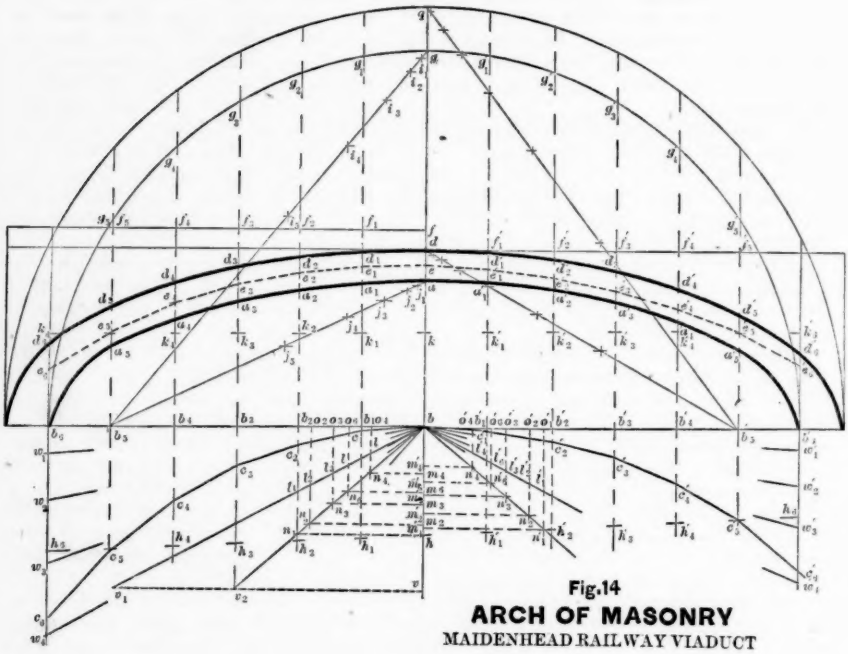


Fig.14

ARCH OF MASONRY

MAIDENHEAD RAILWAY VIADUCT

doubted. Rankine, however, in his Applied Mechanics assumes that the pressures are of an amount and in a direction due to the conjugate stresses of an homogeneous, elastic material, or of a material which like earth has an angle of slope due to internal friction. While this is a correct assumption, in case of the arch of a tunnel sustaining earth, it is incorrect for the case in hand, for the masonry of the surcharge needs only a vertical resistance to support it, and will of itself produce no active thrust, having a horizontal component.

This is further evident from Moseley's principle of least resistance, which is stated and proved by Rankine in the following terms.

"If the forces which balance each other in or upon a given body or structure, be distinguished into two systems, called respectively, *active* and *passive*, which stand to each other in the relation of cause and effect, then will the passive forces be the least which are capable of balancing the active forces, consistently with the physical condition of the body or structure.

For the passive forces being caused by the application of the active forces to the body or structure, will not increase after the active forces have been balanced by them; and will, therefore, not increase beyond the least amount capable of balancing the active forces."

A surcharge of masonry can be sustained by vertical resistance alone, and therefore will exert of itself a pressure in no other direction upon the haunches of the arch. Nevertheless this surcharge will afford a resistance to horizontal pressure if produced by the arch itself. So that when we assume the pressures due to the surcharge to be vertical alone, we are assuming that the arch does not avail itself of one element of stability which may possibly be employed, but which the engineer will hesitate to rely upon, by reason of the inferior character of the masonry usually found in the surcharge. The difficulty is usually avoided, as in that beautiful structure, the London

Bridge, by forming a reversed arch over the piers which can exert any needed horizontal pressure upon the haunches. This in effect increases by so much the thickness of the arch ring at and near the piers.

The pressure of earth will be treated in connection with the construction for the Retaining Wall. On combining the pressures there obtained with the weight, the load which a tunnel arch sustains, may be at once found, after which the equilibrium polygon may be drawn and a construction executed, similar in its general features to that about to be employed in the case before us.

Let us assume that the arch is loaded with a live load extending over the left half of the span, and having an intensity which when reduced to masonry of the same specific gravity as that of which the viaduct is built, would add a depth df to the surcharge. Now if the number of parts into which the span is divided be considerable, the weights which may be supposed to be concentrated at the points of division vary very approximately as the quantities of the type af. This approximation will be found to be sufficiently exact for ordinary cases; but should it be desired to make the construction exact, and also to take account of the effect of the obliquity of the joints in the arch ring, the reader will find the method for obtaining the centers of gravity, and constructing the weights, in Woodbury's Treatise on the Stability of the Arch pp. 405 *et seq.* in which is given Poncelet's graphical solution of the arch.

With any convenient pole distance, as one half the span, lay off the weights. We have used b as the pole and made $b_0w_1 = \frac{1}{2}$ the weight at the crown $= \frac{1}{4}(af + ad) = b_0'w_1$, $w_1w_2 = a_1f_1$, $w_2w_3 = a_2f_2$, etc. Several of the weights near the ends of the span are omitted in the Figure; viz., w_4w_5, etc. From the force polygon so obtained, draw the equilibrium polygon c as previously explained.

The equilibrium polygon which expresses the real relations between the loading and the thrust along the arch, is evidently one whose ordinates are proportional to the ordinates of the polygon c.

It has been shown by Rankine, Woodbury and others, that for perfect stability, —*i.e*, in case no joint of the arch begins to open, and every joint bears over its entire surface,—that the point of application of the resultant pressure must everywhere fall within the middle third of the arch ring. For if at any joint the pressure reaches the limit zero, at the intrados or extrados, and uniformly increases to the edge farthest from that, the resultant pressure is applied at one third of the depth of the joint from the farther edge.

The locus of this point of application of the resultant pressure has been called the "curve of pressure," and is evidently the equilibrium curve due to the weights and to the actual thrust in the arch. If then it be possible to use such a pole distance, and such a position of the pole, that the equilibrium polygon can be inscribed within the inner third of the thickness of the arch ring, the arch is stable. It may readily occur that this is impossible, but in order to ensure sufficient stability, no distribution of live load should be possible, in which this condition is not fulfilled.

We can assume any three points at will, within this inner third, and cause a projection of the polygon c to pass through them, and then determine by inspection whether the entire projection lies within the prescribed limits. In order to so assume the points that a new trial may most likely be unnecessary, we take note of the well known fact, that in arches of this character, the curve of pressure is likely to fall without the prescribed limits near the crown and near the haunches. Let us assume e at the middle of the crown, e_0' at the middle of $a_0'd_0'$, and e_0 near the lower limit on a_0d_0. This last is taken near the lower limit, because the curvature of the left half of the polygon is more considerable than the other, and so at some point between it and the crown may possibly rise to the upper limit. The same consideration would have induced us to raise e_0' to the upper limit, were it not likely that such a procedure would cause the polygon to rise above the upper limit on the right of e_0'.

Draw the closing line kk through e_0e_0', and the corresponding closing line hh through c_0c_0', and decrease all the ordinates of the type hc in the ratio of hb to ke, by help of the lines bn and bl, in a

manner like that previously explained. For example $h_3c_3=n_3o_3$, and $l_3o_3=k_3e_3$. By this means we obtain the polygon e which is found to lie within the required limits. The arch is then stable: but is the polygon e the actual curve of pressures? Might not a different assumption respecting the three points through which it is to pass lead to a different polygon, which would also lie within the limits? It certainly might. Which of all the possible curves of pressure fulfilling the required condition, is to be chosen, is determined by Moseley's principle of least resistance, which applied to the case in hand, would oblige us to choose that curve of all those lying within the required limits, which has the least horizontal thrust, *i.e.* the smallest pole distance. It appears necessary to direct particular attention to this, as a recent publication on this subject asserts that the true pressure line is that which approaches nearest to the middle of the arch ring, so that the pressure on the most compressed joint edge is a minimum; a statement at variance with the theorem of least resistance as proved by Rankine.

Now to find the particular curve which has the least pole distance, it is evidently necessary that the curve should have its ordinates as large as possible. This may be accomplished very exactly, thus: above e_1 where the polygon approaches the upper limit more closely than at any other point near the crown, assume a new position of e_1 at the upper limit; and below e_4' where it approaches the lower limit most nearly on the right, assume a new position of e_4' at the lower limit. At the left e_5 may be retained. Now on passing the polygon through these points it will fulfill the second condition, which is imposed by the principle of least resistance.

A more direct method for making the polygon fulfill the required condition will be given in Fig. 18.

It is seen in the case before us, the changes are so minute that it is useless to find this new position of the polygon, and its horizontal thrust. The thrust obtained from the polygon e in its present position is sufficiently exact. The horizontal thrust in this case is found from the lines bn and bl. Since $2vv_2$ is the horizontal thrust, *i.e.* pole distance of the polygon e, $2vv_1$ is the horizontal thrust of the polygon e.

By using this pole distance and a pole properly placed, we might have drawn the polygon e with perhaps greater accuracy than by the process employed, but it being the process employed in Figs. 2, 3, etc., we have given this as an example of another process.

The joints in the arch ring should be approximately perpendicular to the direction of the pressure, *i.e.* normal to the curve of pressures.

With regard to what factor of safety is proper in structures of this kind, all engineers would agree that the material at the most exposed edge should never be subjected to a pressure greater than one fifth of its ultimate strength. Owing to the manner in which the pressure is assumed to be distributed in those joints where the point of application of the resultant is at one third the depth of the joint from the edge, its intensity at this edge is double the average intensity of the pressure over the entire joint. We are then led to the following conclusion, that the total horizontal thrust (or pressure on any joint) when divided by the area of the joint where this pressure is sustained ought to give a quotient at least ten times the ultimate strength of the material. The brick viaduct which we have treated is remarkable in using perhaps the smallest factor of safety in any known structure of this class, having at the most exposed edge a factor of only $3\frac{1}{2}$ instead of 5.

It may be desirable in a case like that under consideration, to discuss the changes occuring during the movement of the live load, and that this may be effected more readily, it is convenient to draw the equilibrium polygons due to the live and dead loads separately. The latter can be drawn once for all, while the former being due to a uniformly distributed load can be obtained with facility for different positions of the load. The polygon can be at once combined into a single polygon by adding the ordinates of the two together. Care must be taken, however, to add together only such as have the same pole distance. In case the construction which has been given should show that the arch is unstable, having no projection of the equilibrium polygon which can be inscribed

within the middle third of the arch ring, it is possible either to change the shape of the arch slightly, or increase its thickness, or change the distribution of the loading. The last alternative is usually the best one, for the shape has been chosen from reasons of utility and taste, and the thickness from consideration of the factor of safety. If the center line of the arch ring (or any other line inscribed within the middle third) be considered to be an equilibrium polygon, and from a pole, lines be drawn parallel to the segments of this polygon, a weight line can be found which will represent the loading needed to make the arch stable. If this load line be compared with that previously obtained, it will be readily seen where a slight additional load must be placed, or else a hollow place made in the surcharge, such as will render the arch stable. In general, it may be remarked, that an additional load renders the curvature of the line of pressures sharper under it, while the removal of any load renders the curve straighter under it.

The foregoing construction is unrestricted, and applies to all unsymmetrical forms of arches or of loading, or both. As previously mentioned, a similar construction applies to the case of an arch sustaining the pressure of water or earth; in that case, however, the load is not applied vertically and the weight line becomes a polygon.

RETAINING WALLS AND ABUTMENTS.

Let $aa'b'b$ in Fig. 15 represent the cross section of a wall of masonry which retains a bank of earth having a surface aa_6. Assume that the portion of the wall and earth under consideration is bounded by two planes parallel to the plane of the paper, and at a unit's distance from each other: then any plane containing the edge of the wall at b, as ba_6, ba_1, etc., cuts this solid in a longitudinal section, which is a rectangle having

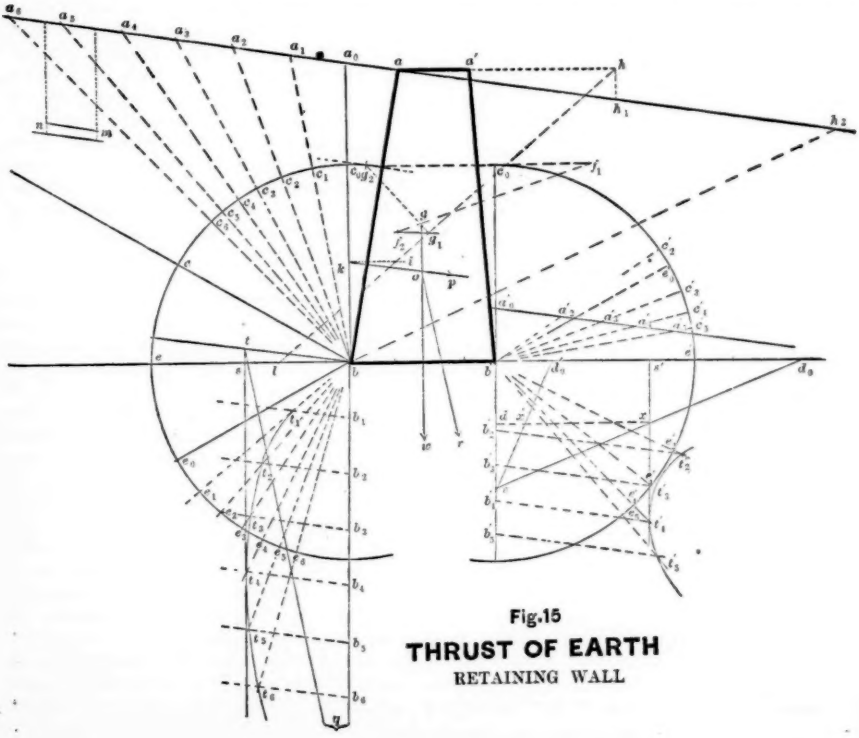


Fig.15

THRUST OF EARTH

RETAINING WALL

a width of one unit, and a length ba_0, ba_1, etc.

The resultant of the total pressure distributed over any one of these rectangles of the type ba is applied at one-third of that distance from b: *i.e.* the resultant pressure exerted by the earth against the rectangle at ba_0 is applied at a distance of $bk=\frac{1}{3}\,ba_0$ from b.

That the resultant is to be applied at this point, is due to the fact that the distributed pressure increases uniformly as we proceed from any point a of the surface toward b: the center of pressure is then at the point stated, as is well known.

Again, the direction of the pressures against any vertical plane, as that at ba_0, is parallel to the surface aa_3. This fact is usually overlooked by those who treat this subject, and some arbitrary assumption is made as to the direction of the pressure.

That the thrust of the earth against a vertical plane is parallel to the ground surface is proved analytically in Rankine's Applied Mechanics on page 127; which proof may be set forth in an elementary manner by considering the small parallelopiped mn, whose upper and lower surfaces are parallel to the ground surface. Since the pressure on any plane parallel to the surface of the ground is due to the weight of the earth above it, the pressure on such a plane is vertical and uniformly distributed. If mn were a rigid body, it would be held in equilibrium by these vertical pressures, which are, therefore, a system of forces in equilibrium; but as mn is not rigid it must be confined by pressures distributed over each end surface, which last are distributed in the same manner on each end, because each is at the same depth below the surface. Now the vertical pressures and end pressures hold mn in equilibrium: they therefore form a system in equilibrium. But the vertical pressures are independently in equilibrium, therefore the end pressures alone form a system which is independently in equilibrium. That this may occur, and no couple be introduced, these must directly oppose each other; *i.e.* be parallel to the ground line aa_3.

Draw $kp \parallel aa_3$, it then represents the position and direction of the resultant pressure upon the vertical ba_0. Draw the horizontal ki, then is the angle ikp called the *obliquity* of the pressure, it being the angle between the direction of the pressure and the normal to the plane upon which the pressure acts.

Let $ebc=\Phi$ be the *angle of friction*, *i.e.* the inclination which the surface of ground would assume if the wall were removed.

The obliquity of the pressure exerted by the earth against any assumed plane, such as ba_2 or ba_3, must not exceed the angle of friction; for should a greater obliquity occur the prism of earth, a_0ba_2 or a_0ba_3, would slide down the plane, ba_2 or ba_3, on which such obliquity is found.

For dry earth Φ is usually about 30°; for moist earth and especially moist clay, Φ may be as small as 15°. The inclination of the ground surface aa_3 cannot be greater than Φ.

Now let the points a_1, a_2, a_3, etc., be assumed at any convenient distances along the surface: for convenience we have taken them at equal distances, but this is not essential. With b as a center and any convenient radius, as bc, describe a semi-circumference cutting the lines ba_1, ba_2, etc. at c_1, c_2, etc. Make $ee_0=ec$; also $e_0e_1=c_0c_1$, $e_0e_2=c_0c_2$, etc.: then be_0 has an obliquity Φ with ba_0, as has also be_1 with ba_1, be_2 with ba_2, etc.; for $a_0be_0=a_1be_1=a_2be_2=90°+\Phi$.

Lay off bb_1, bb_2, bb_3, etc., proportional to the weights of the prisms of earth a_0ba_1, a_0ba_2, a_0ba_3, etc.: we have effected this most easily by making $a_0a_1=bb_1$, $a_0a_2=bb_2$, $a_0a_3=bb_3$, etc. Through b, b_1 b_2, etc. draw parallels to ki; these will intersect be_0, be_1, be_2, etc., at b, t_1, t_2, etc. Then is bb_1t_1 the triangle of forces holding the prism a_0ba_1 in equilibrium, just as it is about to slide down the plane ba_1, for bb_1 represents the weight of the prism, b_1t_1 is the known direction of the thrust against ba_0, and bt_1 is the direction of the thrust against ba_1 when it is just on the point of sliding: then is t_1b_1 the greatest pressure which the prism can exert against ba_0. Similarly t_2b_2 is the greatest pressure which the prism a_0ba_2 can exert. Now draw the curve $t_1t_2t_3$, etc., and a vertical tangent intersecting the parallel to the surface through b at t; then is tb the greatest pressure which the earth can exert against ba_0. This greatest pressure is exerted approximately by the prism or wedge of earth cut off by the plane ba_4, for the pressure which it exerts against the vertical plane

through b is almost exactly $b_4t_4=bt$. This is Coulomb's "wedge of maximum thrust" correctly obtained: previous determinations of it have been erroneous when the ground surface was not level, for in that case the direction of the pressure has not been ordinarily assumed to be parallel to the ground surface.

In case the ground surface is level the wedge of maximum thrust will always be cut off by a plane bisecting the angle cbc_0, as may be shown analytically, which fact will simplify the construction of that case, and enable us to dispense with drawing the thrust curve tt.

The pressure tb is to be applied at k, and may tend either to overturn the wall or to cause it to slide.

In order to discuss the stability of the wall under this pressure, let us find the weight of the wall and of the prism of earth aba_0. Let us assume that the specific gravity of the masonry composing the wall is twice that of earth. Make $a'h=bb'$, then the area $abb'a'=abh=abh_1$; and if $ah_2=2ah$, then ah_2 represents the weight of the wall reduced to the same scale as the prisms of earth before used. Since aa_0 is the weight of aba_0, a_0h_2 is the weight of the mass on the right of the vertical ba_0 against which the pressure is exerted.

Make $bq=a_0h_2$, and draw tq, which then represents the direction and amount of the resultant to be applied at o where the resultant pressure applied at k intersects the vertical gw through the center of gravity g of the mass $aa_0bb'a'$. The center of gravity g is constructed in the following manner. Lay off $a'h=bb'$, and $bl=aa'$; and join hl. Join also the middle points of ab and $a'b'$: the line so drawn intersects hl at g_1 the center of gravity of $aa'b'b$. Find also the center of gravity g_2, of aba_0, which lies at the intersection of a line parallel to aa_0, and cutting ba_0 at a distance of $\frac{1}{3}ba_0$ from a_0, and of a line from b bisecting aa_0. Through g_2 and g_1 draw parallels, and lay off g_2f_1 and g_1f_2 on them proportional to the weights applied at g_1 and g_2 respectively. We have found it convenient to make $g_2f_1=\frac{1}{2}ah_2$, and $g_1f_2=\frac{1}{2}aa_0$. Then f_1f_2 divides g_1g_2 inversely as the applied weights; and g, the point of intersection, is the required center of gravity.

Let or be parallel to tq; since it intersects bb' so far within the base, the wall has sufficient stability against overturning. The base of the wall is so much greater than is necessary for the support of the weight resting upon it, that engineers have not found it necessary that the resultant pressure should intersect the base within the middle third of the joint. The practice of English engineers, as stated by Rankine, is to permit this intersection to approach as near b' as $\frac{1}{8}bb'$, while French engineers permit it to approach as near as $\frac{1}{6}bb'$ only. In all cases of buttresses, piers, chimneys, or other structures which call into play some fraction of the ultimate strength of the material, or ultimate resistance of the foundation as great as one tenth, or one fifteenth, the point should not approach b' nearer than $\frac{1}{3}bb'$.

Again, let the angle of friction between the wall and the earth under it be Φ': then in order that the thrust at k may not cause the wall to slide, the angle wor must be less than Φ'.

When, however, the angle Φ' is less than wor it becomes necessary to gain additional stability by some means, as for example by continuing the wall below the surface of the ground lying in front of it. Let $a_0'a_5'$ be the surface of the ground which is to afford a *passive* resistance to the thrust of the wall: then in a manner precisely analogous to that just employed for finding the greatest active pressure which earth can exert against a vertical plane, we now find the least passive pressure which the earth in front of the wall will sustain without sliding up some plane such as $b'a_3'$ or $b'a_4'$, etc. The difference in the two cases is that in the former case friction hindered the earth from sliding down, while it now hinders it from sliding up the plane on which it rests.

Lay off $e'e_0'=ee_0$; then taking any points $a_2'a_3'$, etc. on the ground surface, make $e_0'e_2'=c_0'c_2'$, $e_0'e_3'=c_0'c_3'$, etc.

Lay off $b'b_2'=a_0'a_2'$, etc., and drawing parallels through b_2', b_3', etc., we obtain the thrust curve $t_2't_3'$, etc.

The small prism of earth between $b'a_0'$ and the wall adds to the stability of the wall, and can be made to enter the construction if desired, in the same manner as did aba_0.

The vertical tangent through s' shows us that the earth in front of the wall can

withstand a thrust having a horizontal component $b's'$ measured on a scale such that $b'b_2'=a_0'a_2'$ is the weight of the prism of earth $a_0'b'a_2'$.

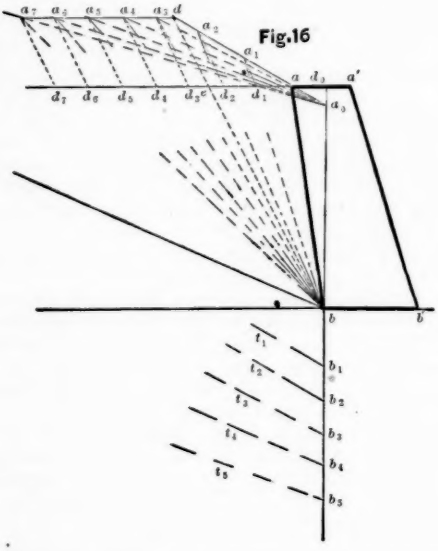
This scale is different from that used on the left. To reduce them to the same scale lay off from b', the distances $b'd_0$ and $b'd_0'$ proportional to the perpendiculars from b on aa_4 and b' on $a_1'a_4'$ respectively. In the case before us, as the ground surfaces are parallel, we have made $b'd_0=ba_0$ and $b'd_0'=b'a_0'$.

Then from any convenient point on $b'b_4'$, as v, draw vd_0 and vd_0': these lines will reduce from one scale to the other. We find then that $x'd$ is the thrust on the scale at the left corresponding to $xd=b's'$ on the right: *i.e.*, the earth under the surface assumed at the right can withstand something over one fourth of the thrust sb at the left.

It will be found that a certain small portion of the earth near a_0' has a thrust curve on the left of b', but as it is not needed in our solution it is omitted.

If any pressure is required in pounds, as for example sb, it is founds as follows: —the length of ah_2 is to that of sb as the weight of $bb'aa'$ in lbs. is to the pressure sb in lbs.

Frequently the ground surface is not a plane, and when this is the case it often consists of two planes as ad, da, Fig. 16.



In that case, draw some convenient line as ad_1, and lay off ad_1, d_1d_2, etc. at will, which for convenience we have made equal. Draw d_1a_1, d_2a_2, etc. parallel to bd, and join ba_1, ba_2, etc.: then are the triangles bda, bda_1, bda_2, bda_3, etc. proportional in area to the lines ea, ea_1, etc. Hence the weights of the prisms of earth baa_1, baa_2, etc., are proportional to ad_1, ad_2, etc.

In case ab slopes backward the part of the wall at the left of the vertical ba_0 rests upon the earth below it sufficiently to produce the same pressure which would be produced if baa_0 were a prism of earth. The weights of the wedges which produce pressures, and which are to be laid off below b, are then proportional to $d_0d_1=bb_1$, $d_0d_2=bb_2$, etc. The direction of the pressures of the prisms at the right of bd are parallel to ad; but upon taking a larger prism the direction may be assumed to be parallel to a_0a_3, a_0a_4, etc., which is very approximately correct. Now draw $b_1t_1 \parallel a_0a_1$, $b_4t_4 \parallel a_0a_4$, etc.; and complete the construction for pressure precisely as in Fig. 15, using for resultant pressure the direction and

amount of that due to the wedge of maximum pressure thus obtained.

In finding the stability of the wall, it will be necessary to find the weight and center of gravity of the wall itself, minus a prism of earth baa_6, instead of plus this prism as in Fig. 15; for it is now sustained by the earth back of the wall.

When the back of the wall has any other form than that above treated, the vertical plane against which the pressure is determined should still pass through the lower back edge of the wall.

In case the wall is found to be likely to slide upon its foundations when these are level, a sloping foundation is frequently employed, such that it shall be nearly perpendicular to the resultant pressure upon the base of the wall. The construction employed in Fig. 15 applies equally to this case.

The investigation of the stability of any abutment, buttress, or pier, against overturning and against sliding, is the same as that of the retaining wall in Fig. 15. As soon as the amount, direction, and point of application, of the pressure exerted against such a structure is determined, it is to be treated precisely as was the resultant pressure kp in Fig. 15.

In the case of a reservoir wall or dam, the construction is simplified from the fact that, since the surface of water is level and the angle of friction vanishes, the resultant pressure is perpendicular to the surface upon which the water presses. It is useful to examine this as a case of our previous construction. In Fig. 17, let abb' be the cross-section of

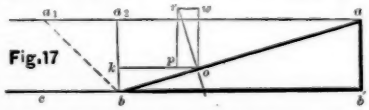


Fig. 17

the dam; then the wedge of maximum pressure against ba_6 is cut off by the plane ba_1 when $cba_1 = 45°$, *i.e.* ba_1 bisects cba_6 as before stated.

This produces a horizontal resultant pressure at k equal to the weight of the wedge. Now the total pressure on ab is the resultant of this pressure, and the weight of the wedge aba_6. The forces to be compounded are then proportional to the lines $a_1a_6 = bv_6$ and aa_6. By similarity of triangles it is seen that ro the resultant is perpendicular to ab.

It is seen that by making the inclination of ab small, the direction of ro can be made so nearly vertical that the dam will be retained in place by the pressure of the water alone, even though the dam be a wooden frame, whose weight may be disregarded.

We can now construct the actual pressures to which the arch of a tunnel surcharged with water or earth is subjected. Suppose, for example, we wish to find the pressure of such a surcharge on the voussoir $a_4d_4d_5a_5$ Fig. 14. Find the resultant pressure against a vertical plane extending from d_5 to the upper surface of the surface and call it p_5. Draw a horizontal through d_4 and let its intersection with the vertical just mentioned be called d_5''. Find the resultant pressure against the vertical plane extending from d_5'' to the surface, and call it p_5'. Now let $p_5'' = p_5 - p_5'$ and let it be applied at such a point of d_5d_5'' that p_5 shall be the resultant of p_5' and p_5''. Then will the resultant pressure against the voussoir be the resultant of p_5'' and the weight of that part of the surcharge directly above it.

FOUNDATIONS IN EARTH.

A method similar to that employed in the determination of the pressure of earth against a retaining wall, or a tunnel arch, enables us to investigate the stability of the foundations of a wall standing in earth.

Suppose in Fig. 15 that the wall $abb'a'$ is a foundation wall, and that the pressure which it exerts upon the plane bb' is vertical, being due to its own weight and the weight of the building or other load which it sustains. Now consider a vertical plane of one unit in height, say, as bb_1; and determine the resultant pressure against it on the supposition that the pressure is produced by a depth of earth at the right of it, sufficient to produce the same vertical pressure on bb' which the wall and its load do actually produce. In other words we suppose the wall and load replaced by a bank of earth having its upper surface horizontal and weighing the same as the wall and load. Call the upper surface z, and find the pressure against the vertical plane zb due to the earth under the given level surface; similarly, find the pressure

against zb_1. The surface being level, the maximum pressure, as previously stated will be due to a wedge cut off by a plane besecting the angle between bz and a plane drawn from b at the inclinatian Φ, of the limiting angle of friction. This enables us to find the horizontal pressures against zb and zb_1 directly: their difference is the resultant active pressure against bb_1.

Next, it must be determined what passive pressure the earth at the left of bb_1 can support. The passive resistance of the earth under the surface a against the plane ab as well as that against the plane ab_1 can be found exactly as that was previously found under the surface a'. The difference of these resistances is the resistance which it is possible for bb_1 to support. Indeed bb_1 could support this pressure and afford this resistance even if the active pressure against ab were, at the limit of its resistance, which it is not. The limiting resistance which is thus obtained, is then so far within the limits of stability, that ordinarily, no further factor of safety is needed, and the stability of the foundation is secured, if the active pressure against bb_1 does not exceed the passive resistance. This construction should be made on the basis of the smallest angle of friction Φ which the earth assumes when wet; that being smaller than for dry earth, and hence giving a greater active pressure at the right, and a less resistance at the left.

ON WELDING.

By RICHARD HOWSON.

Journal of the Iron and Steel Institute.

In the paper which I had the honor to read at the last meeting of this Institution, I endeavoured to show that the rationale of welding, although it might be esteemed trivial in itself, had really a very important bearing on the subject of puddling, especially puddling by machinery on a large scale. In the discussion which followed some criticisms fell from members, which induce me to claim the indulgence of the meeting for a few additional remarks, in order more clearly to elucidate the views then brought forward.

I drew attention on that occasion to the fact that the process of manufacturing wrought iron was, from beginning to end, a process of welding, whether it is effected by successive doubling, or whether the so-called homogeneous process is adopted.

In both cases it was maintained that the presence of silica, or some equivalent flux, was an advantage in the whole series of operations, in the same manner that it is of use in uniting two pieces of iron in a smith's forge.

Mr. Bell took the ground, which others have also done, that a flux is not necessary. He instanced in the first place the case of platinum, which unites of itself at a certain temperature by pure metallic contact. Now nothing could better confirm the view which I then took than this example. Platinum is an unoxidisable metal, and, consequently, during the process of heating, no film of oxide is formed on its surface to interfere with its proper union. Where there is nothing present which requires fluxing, a flux would be quite superfluous. Iron, on the contrary, is a highly oxidisable metal, and its oxide, in common with those of all metals, is of a very refractory character when pure, although its affinities are such that a very small addition of silica imparts to it comparative fusibility.

This leads to the second remark of Mr. Bell, that it is not necessary in welding to add any flux, that iron in fact can be welded without the use of sand. In reference to this observation I can only refer to my former remarks, the whole course of which distinctly implied that the outer skin of wrought iron and the interstices between its fibres consisted of cinder (where cinder was present), which was not a pure oxide but a basic silicate, which is equivalent to saying that it carried its own flux. In all the processes of manufacturing finished iron by piling, it

is certain that no flux is added, but this is because sufficient is already present, being derived from the silicon contained in the pig iron, and from the silica and phosphoric acid which are always more or less mixed with the fettling of the puddling furnace. If I dwelt at unnecessary length on the frequent use of sand in the operations of forging, it was only for the purpose of illustrating the utility of that material, and tracing its mode of action in the larger operations of puddling, hammering, and rolling.

Referring now to the case of platinum, it follows that if cast steel, or any other metal which has been fused and contains no cinder, can be preserved from oxidation during the process of heating—that is to say, if the surfaces to be united can be made to come together in a perfectly clean state, a sound weld ought to be obtained, and the addition of a flux would be not only superflous, but injurious. It is known that two surfaces of a soft metal, such as lead, may be united even cold in this manner. In the case of cast steel, where heat is required, the following experiment is conclusive:—Two pieces of cast steel were planed and filed each on one face perfectly flat and true. The two clean surfaces were then placed on one another; the pieces were bound together with soft iron wire, and the edges luted round with fire-clay to prevent the entrance of air. After being brought in a smith's fire to a sub-white heat, the two were united with half-a-dozen blows of a sledge hammer. The weld is perfect—so thorough, indeed, that it cannot be detected when broken through. Its course can only be guessed at by following the indications on the rough outside edges.

We have here an instance of union exactly parallel to that of platinum, by unalloyed metallic contact. On the other hand, in order to exhibit the influence of an intervening refractory oxide, the following experiment was tried:—Two pieces of cast steel were prepared by planing and filing exactly as before, but the two level surfaces were painted with a thin film of jeweler's rouge, which is a tolerably pure form of peroxide of iron. A small well was also drilled in the middle of one piece, which was filled with the same rouge. The same process of binding, luting, heating, and hammering was then carried out, and the result is a bad weld, or rather no weld at all.*

On splitting open the two faces, which was readily done with a chisel, and examining their condition, the oxide in the well appears to have been imperfectly fused, while there are indications that the thin film has become partially reduced to the state of wrought iron by the action of the carbon in the steel, the oxygen no doubt escaping. However this may be, the experiment affords an exact parallel to what takes place when the attempt is made to weld two pieces of cast steel, by heating them separately in a fire without any flux. In the latter case, the oxide formed by the action of the blast would be quite as pure as the rouge which in the experiment was applied artificially, it would be equally refractory, and there would be more of it. The utility of a flux is here self-evident, and need not be dwelt on further. In the case of wrought iron, as already explained, there is not the same necessity for its employment, as it is already present in sufficient quantity, unless the iron is of an unusually dense character.

Referring now shortly to the subject of machine puddling, I propose only to consider it strictly in its relation to the foregoing conclusions. The more refined chemistry of the process I leave to abler hands.

The most suitable lining for a revolving furnace, in fact the only admissible one, so far as is known at present, is the purest oxide of iron that can be procured. It is, indeed, somewhat surprising to find a single material combining in itself so many good properties, and that no substance is capable of replacing it in its chemical adaptation for eliminating all the impurities of pig iron. It is, however, highly refractory, and this refractoriness, although an advantage in one repect, acts unfavorably on the final result. In charging the furnace, a small quantity of impure silicious cinder is usually thrown in, so as to assist in procuring a certain degree of fusibility, and enable the iron to revolve in a liquid bath; but it is generally considered that, the purer the cinder the better is the puddled product. Hence comes the practice of ap-

* [NOTE].—I am indebted to Mr. Newcomb, the Secretary of the Cleveland Iron Trade Foremen's Association, for his great care and skill in manipulating these tests, which may be thoroughly relied on.

plying an intense heat, by which means alone a sufficient liquefaction of the fettling is procured in order to do its work effectively. The consequence of this is that, although the iron may be thoroughly puddled when the heat is finished, the ball contains a cinder which, being comparatively infusible, sets readily on exposure to the cold atmosphere. I have before pointed out that shingling a puddled ball is essentially a process of welding, and here comes into play the fact that a ball will not shingle satisfactorily, especially if of large size, unless its contained cinder has a certain degree of liquidity. In order to procure a fine quality of iron a pure fettling is employed, and the heat is urged up to a point which rapidly destroys the furnace, while it often happens, that when the ball comes to the hammer, it drops in pieces in consequence of the setting of the cinder before it can be squeezed together.

It is true that the same untoward event will sometimes occur in ordinary puddling, but this is on a smaller scale, and the puddler usually takes care of his heat, which does not affect his furnace injuriously to any great extent; besides which, if it did, one furnace spoilt, he would ask for another, which he sometimes does. The destructive action of a high temperature on the revolving furnace is also accompanied by another evil—viz., an excessive and costly consumption of fettling, owing to the entire inner circumference being exposed to flame.

These are some of the difficulties of machine puddling, but I believe they are the only really formidable difficulties. The common process of puddling has been established by long usage, but we should not forget that its introduction also was beset with many troubles. We have only to glance over the abstracts of patents, from the period when sand bottoms were used up to the present, to note the variety of schemes which have been attempted to improve it. Among these inventions there is one class which I would here refer to, as it is kindred to the subject in hand—viz., that of chemical mixtures, commonly called physic. On examining this long series of specifications of inventions applicable to puddling, almost every element will be found named, and innumerable combinations, supposed by the patentees (as it is usually expressed) to purify the iron. Many of these nostrums are absurd enough; others are of problematical value. There are, however, doubtless, a great number which are more or less useful, but, with the exception of those which are intended to give the iron a steely nature, it may be questioned whether any of them act chemically on the metal to improve its quality beyond that which the fettling alone can effect without admixture. Their utility probably consists simply in this, that they assist in imparting that fluidity to the cinder which is so essential in giving soundness to the hammered bloom.

According to the instructions usually specified in these inventions, the proper time for adding the physic seems to be towards the end of the process, and there is good reason for this, because if it were applied early it would tend to scour the fettling, whereas, if applied late, it unites at once with the cinder which is already melted, a great part of which will be carried away by the puddled balls. It is a question whether the adoption of this method of working in revolving furnaces would not enable the excessive heat to be somewhat moderated. A few handfuls of sand thrown in just at the point when the iron is ready to ball up, always facilitates the shingling process, and tends to produce a solid bloom, without affecting the iron, so far as I am aware, injuriously.

In reference, therefore, to the horror usually expressed at the mention of sand in the puddling furnace, it ought to be distinctly understood that it is only objectionable in the process for the very same reason that it is beneficial in the ball, viz.: that it promotes fusibility. In the former case, it acts destructively on the lining, and in the latter, it facilitates the union of the particles of iron. If it be supposed that the metal itself suffers detriment from its presence, the two following experiments, communicated to me by Mr. Stead (of the firm of Pattinson and Stead), and suggested originally, I believe, by Mr. Williams, seem to affirm the contrary.

A small quantity of Cleveland pig, accompanied with some cinder, was poured in a liquid state into a hot cruci-

ble; a cover was then put on the crucible, and the whole was then shaken up violently for a quarter of a minute.

No. 1 experiment, the cinder consisted of mill tap alone. In No. 2, it consisted of eighty per cent. mill tap and twenty per cent. sand. The analysis of the metal after each operation was as follows:

	No. 1. Per Cent.	No. 2. Per Cent.
Carbon	2.40	3.00
Silicon	Trace.	Trace.
Phosphorus	0.13	0.04

It appears that, in eliminating the phosphorus, where the real difficulty lies, the best result was obtained where sand was present. It may here be noted that the wonderful short time in which this elimination was effected from pig containing not less than 1.25 per cent. of phosphorus, confirms the fact, if confirmation were needed, that agitation, not less than heat, is an essential of effective puddling. Hence it is again a matter worth consideration, whether more perfect mechanical agitation may not to some extent be a substitute for, and tend so far to moderate, the excessive and destructive heat.

Machine puddling in the revolving furnace has during the last few years been on its trial, and has had a hard struggle for the mastery, to which it is still destined to attain. I have endeavored to indicate where the great difficulties lie, and how they may be to some extent obviated by varying the mode of working, but it is quite possible still more to obviate them by improvement in construction. With regard to the practicability of producing finished iron without piling, I have only to refer to the results which Messrs. Hopkins, Gilkes, & Co. have obtained, to show that at all events, with certain classes of manufacture, this point may be considered established. Whether it will be found expedient to work in masses of extremely large dimensions, or not, is a question which only the construction of costly machinery can decide.

INFLUENCE OF FORM ON THE MAGNETISM OF SOFT IRON CYLINDERS.

From "The Engineer."

THOUGH the magnetism of soft iron is a subject that has been largely investigated, it has not been sufficiently recognised that the various experimental researches upon it, not having, for the most part, been made in a homogeneous magnetic field, are not directly comparable with one another, and so the most of the laws laid down are valid only for the special arrangements of a particular experiment. Further—and this objection proves to be a most important one—the researches hitherto have not been sufficiently comprehensive, and more especially, they have not been made with iron masses whose dimensions varied within wide limits. Lastly, the remanent magnetism of soft iron has been little examined, owing to the small so-called coercive force generally attributed to this material. These reasons recently induced M. Christoph Ruths to make a thorough investigation of the magnetism produced in soft iron cylinders of the most various dimensions, by increasing magnetising force in a homogeneous field, and of the magnetism which remained after the removal of the induction. The results have been published in a monograph by the author, which has recently appeared in Dortmund. The following is an abstract of M. Ruths' valuable paper:—As material for the iron cylinder to be magnetised, commercial iron wire was taken, obtained from the same manufactory. Seven kinds, of different thickness, were examined, their radii being, on an average, 2.54, 1.67, 1.39, 0.81, 0.62, 0.44, and 0.31mm. Of these different thicknesses pieces were prepared, whose lengths in millimetres were 200, 190, 180, 170, 160, 150, 140, 120, 100, 80, 60, and 40; further iron cylinders were procured, the radii of which were 6.5, 5.65, 4.87, 4.02, 3.65, and 2.75mm. Altogether seventy bars were examined,

whose dimensions varied from nearly cubical form to the most extended forms. Their specific gravity was on an average 7.794. All the cylinders were, before experiment, annealed three times. To produce the induction three separate iron wire spirals were used, the dimensions of which were so large, that for most of the rods a perfectly homogeneous field might be assumed. The iron bars were held by cork in the middle of the spirals, and currents of one to eight Bunsen elements were employed. The intensity of the inducing current was determined with a tangent compass; the magnetic moment with a Wiedemann mirror compass. The bars above referred to were nearly all examined with all three spirals, and on an average, in each series of experiments, there were ten separate experiments. For the special arrangement of the experiments, and the order of operations, the precautions taken, and the mode of calculating the various values, we must refer to the original; merely remarking, that all the results of experiment, the magnetising forces as well as the magnetic moments, are expressed as multiples of the horizontal components of the terrestrial magnetic force, which is assumed to alter but little in the course of the researches. We can only here give the principal results of the experiments. If we denote as induced magnetism the whole magnetism manifested externally by a bar during the action of a magnetising force, the experiments first show that the moment induced by any magnetising force in the unit of weight is a constant function of the dimension-relation a (a = relation of length to thickness), and that to equal magnetism of unit weight of different bars magnetising forces correspond, which are likewise a constant function of a. In bars of the same dimension-relations, equal magnetisms of unit weight always correspond to equal magnetising forces. If we make the curve of magnetisation of soft iron according to the values obtained, we find that for the beginning the quotient of entire magnetism and the magnetising force does not begin with o, but with a determinate value which increases with the dimension-relation a; the value of this quotient then increases to a maximum, and the increase is completed the sooner, the greater the dimension-relation of the bar. The occurrence of the turning point also depends on a, and the magnetising force with which this turning point, *i. e.*, the maximum of the quotient, occurs, is a constant function of the dimension-relation. After reaching the turning point the curve of magnetisation goes at first with a strong, then with a very slight concavity to a maximum value. Whether the latter occurs with a finite or only with an infinitely great magnetising force, must be experimentally determined from further experiments. A study of the question whether and within what limit the induced magnetism may be put proportional to the magnetising force, leads to the result that for bars whose dimension-relation a is very small—say under 12—the quotient of magnetism by magnetising force is approximately constant. It is only for bars of a larger a, that Wiedemann's assertion holds good, that the magnetic movements increase more quickly than the intensities of the currents. Mr. Ruths next compared the foregoing results of experiments with the various theoretically established formulæ for the relation of the induced magnetism to the magnetising current, and found that not one of them corresponds to the ascertained relations. Nor did a simple relation of the induced magnetism to the dimensions of the bar (whether thickness, length, or volume,) appear from the observations. Specially interesting are the facts established by M. Ruths with regard to the remanent magnetising of soft iron after sudden interruption of a current repeatedly sent in the same direction. From the remanent magnetic moment of a bar, a function of the previously acting magnetising force, a curve of magnetisation was constructed, and such curves were obtained for 56 bars, the dimension-relation a of which varied between 20 and 300. All these curves show a constant course throughout. The remanent moments reached, in some bars, values which exceeded $\frac{1}{2}$ of the induced magnetism; and the relation of the remanent to the induced magnetism was greater, the greater the dimension-relation, or the longer the bar. The discussion of the tabulated values of the remanent magnetism shows, that this, with very small magnetising forces, is either *nil*, or has only an im.

perceptible value. With a certain magnetising force, however, which, for the bars examined, varied between 3 and 10, a marked amount of remanent magnetism occurred. After reaching this perceptible value, the remanent moments increase very quickly, and in a greater ratio than the magnetising force, and the induced moment. The curve of magnetisation soon reaches a turning point at which the remanent moment is about $\frac{1}{4}$ of the maximum; this turning point occurs approximately with the same magnetising force. In the further course of the curve, the relation of the remanent moment to the magnetising force reaches a maximum, and the remanent moment then approaches a maximum quickly, and much sooner than the induced magnetism. When this has been reached, there is in general no decrease from it. On the other hand, proportionality was in no case met with between remanent magnetism and magnetising force. A comparisom of the maximum values, according to the dimension relations, shows throughout a decrease of them, with decrease of the length of equally thick bars; with reference to moments of bars of equal length, but different thickness, there seem to be the most manifold varieties. If, however, the maximum remanent moments be arranged according to the dimension-relation a, a conformity to law at once appears. The maximum of remanent magnetism referred to unit weight of a bar is a constant function of the dimension-relation a. This function, with $a=$ about 7, cuts the axis of abscissæ; below this value it is negative, above it positive. Above $a=7$ it rises very quickly, turning its convex side to the axis of abscissæ; this relation then reaches a maximum, and the curve rises in less degree than before. The remanent magnetisms are therefore greater, the longer the bars. The remanent moments in unit weight reached the remarkable value 727, a value which is over 80 per cent. of the corresponding induced moment, and far exceeds all hitherto observed moments. We must not here dwell on the relation of the remanent magnetism to the lengths of the bars, their thickness, and the magnetising force, which are fully discussed by M. Ruths. It may merely be observed that the remanent magnetisms here referred to had quite a stationary character. In long bars, twenty-four hours after induction, the loss of force was only about 3 to 4 per cent., which may very well have been caused by shaking in the process of removing from the spiral. A general survey of the peculiarities of induced magnetism and remanent magnetism, as above indicated, leads to the conclusion that "in direct opposition to the prevailing opinion, according to which the laws of induced magnetism are considered directly transferable to remanent magnetism, the remanent magnetism of soft iron shows a very different behavior from the induced magnetism." This conclusion places remanent magnetism in quite a new light. M. Ruths is going to make it the starting point of new investigations. In an appendix the author considers the magnetism of steel, and shows that similarly, not only the induced, but also the permanent magnetism of steel depends on the dimensions of the bar.

A NEW FORM OF ELECTRIC LIGHT.

From "Engineering."

THE recognized arrangement of the electric light is well known. The carbon points from which the light is obtained are arranged vertically—one above the other, and the distance at which it is required to regulate them in order to produce the greatest effect is governed by clockwork. MM. Jablochkoff and Denayrouze arrange their carbons side by side, separated by a slip of kaolin. It is the combustion of the latter, under the influence of a highly intensified electric current, that gives the light. Thus the system pursued by MM. Jablochkoff and Denayrouze differs considerably from that adopted in the ordinary arrangement of the electric light.

In proceeding to give a more detailed description of this arrangement it may perhaps be as well to trace its gradual progress. The arrangements referred to as tried at Chatham, consisted of two

slips of carbon some 4 inches in length and rather more than a $\frac{1}{4}$ inch in thickness, separated and surrounded by a composition, the whole being moulded into the shape of an ordinary wax or tallow candle. At the experiment in the Louvre this composition was dispensed with; the carbon slips being 4 millimeters in thickness, and whereas in the former trial the carbons differed from each other to a slight extent in thickness, those of the Louvre were of the same size. Each slip of carbon was insulated by a slip of kaolin about 3 millimeters in thickness.

The carbons are held in small brass tubes insulated from each other, and having their lower portions left vacant so as to be readily fixed upon two small rods or thick wires which, as will be explained hereafter, are in connection with the secondary wires of an induction coil. The entire arrangement of the illuminator or candle is bound together at the point where the carbons are set in the brass tubes by a piece of felt or other convenient substance embedded in paste. A further modification consists of overlaying the kaolin with a conducting mixture by which the action of the current is assisted. The kaolin in its cold and solid state is an insulator offering great resistance, but which under the influence of the current becomes hot and melts, and in this liquid condition its resistance is so far reduced as to bring it within the category of a conductor. In order to accelerate its combustion a fine slip of carbon about the size of the lead of a cedar pencil is placed across the kaolin at the top of the carbons, at which point combustion consequently first takes place and continues as the kaolin consumes away. To insure an equal consumption of the carbons it is necessary that the currents of electricity passing through them should be of an opposite character—that is, a positive followed by a negative current. The power of the light obtained at the Louvre from eight of these electric candles was, it is stated, equal to 300 carcel gas burners. In the latter modification, in which the carbon slips are replaced by a conducting composition—the light from which is described as a luminous band—the power of the light can be varied from that equaling two to that of the power of fifteen gas-lights by varying the size of the coils and other evident means. In either case the lights are soft, steady, and brilliantly white, although by mixing with the porcelain clay any coloring matter the light can be colored at pleasure. The consumption of the kaolin or porcelain is about one millimetre an hour.

The electrical arrangement is as follows: The electro-motor, is an ordinary magneto or Gramme machine. In connection with it is the line wire or wires required for providing the current at the point or points at which the lights are required. For every light an induction coil is used, the primary coil of which is in circuit with the line wire, while in the circuit of the secondary coil is placed the candle. On the electro-motor being set in motion the candles in circuit are lighted by the induced current set up from the primary coil. Each light requires a coil, but the coil need not be in the neighborhood of the light. The machine used at the Louvre was of three horse power, giving 450 revolutions per minute. M. Denayrouze remarks that it is possible to relight a candle seven seconds after it has been extinguised by breaking the circuit, and that a great number of mineral substances, and even some organic substances, may be employed to replace the kaolin.

It will be clear from what has been said that lights thus arranged may be put in circuit, and so lit at any time (so long as the motor is in motion) in precisely the same manner as the gaslights of our streets and rooms are brought into use, and that the employment of one and the non-employment of others can in no way interfere with other lights served by the same or by other main line wires. The whole system, indeed, assimilates perfectly with our gas supply. The electro-motor may be regarded as the gasometer, the main wires as the gas mains, and the candles as the burners. To turn the light on it is only necessary to connect the wires, which may be done by a small switch similar to the taps used for the gas branches or chandeliers. The invention is altogether one of considerable merit, and of no small moment. Should M. Jablochkoff succeed in his anticipations, which the experiments at the Louvre give every reason to believe will be the case, he will do much towards revolutionizing our present lighting arrangements.

THE TESTING OF PORTLAND CEMENT.

By ISAAC JOHN MANN.

Proceedings of the Institution of Civil Engineers.

In testing Portland cement it is usual to examine the color, weight, pulverization, and tensile strength. These properties are here enumerated in the inverse order of their importance, the three first being subservient to the last. It is hardly possible to compare accurately the results obtained by different investigators, as the experiments should be made under similar conditions as regards weight, fineness, and method of gauging, but hitherto this uniformity has not been observed.

COLOR.

Good Portland cement, in its dry ungauged condition, is of a uniform dull grey color; occasionally a slightly greenish hue is observable, sometimes replaced by a slight buff tint. A yellow or earthy color is almost invariably indicative of inferior quality. The color can be best observed by pressing a small quantity on a sheet of white paper with a clean smooth trowel, or a piece of sheet glass. But this test alone is insufficient as a guide, as inferior cement may possibly pass it, though good cement rarely, if ever, has the objectionable color referred to.

WEIGHT.

The recorded weight of cement, as at present obtained, depends very much on the manner of weighing. A method, frequently adopted, as giving the most uniform results, is as follows: dry cement, as received from the manufacturer, is allowed to flow from a small hopper-headed shoot into a counterpoised measure of known capacity—usually referred to the imperial bushel—any surplus in the measure being carefully struck off with a light straight-edge; it is then weighed. Instead of a shoot, a vessel shaped like a colander, with holes from $\frac{1}{2}$ to $\frac{3}{4}$ inch in diameter, has also been used. The object in each case is to prevent compression. Weighed after this manner, Portland cement varies from 98 lbs. to 130 lbs. per bushel—equivalent to from 78 lbs. to 102 lbs. per cubic foot. It is difficult to conceive why such a measure of capacity as the bushel (which contains 8 gallons, equivalent to 1.283 cubic foot) should have been selected, as it is not easy of comparison with the usual standards of cubical measurements, the cubic foot being manifestly preferable in every respect.

The weight of cement, ascertained as described or in any analogous manner, bears a very uncertain ratio to its strength, as may be seen from Table 1, which gives the averages of a large number of experiments on cement from different London manufacturers.

TABLE 1.—RELATION BETWEEN THE WEIGHT AND TENSILE STRENGTH OF CEMENT.

Age of Samples seven days; fineness from 6 per cent. to 10 per cent.; time occupied in setting thirty minutes to two hours.

Weight per cubic foot in lbs.	80	81	82	83	84	85	86	87*	88	89
Breaking weight per sq. inch in lbs.	373	353	418	397	399	405	383	463	392	408

* Including some samples or cement of exceptionally high tensile strength.

For convenience of comparison the following tables have been extracted from the Papers submitted to the Institution by Mr. Grant and Mr. Colson (see next page).

The fineness of the cement is not given, and it is apparently assumed that the weight of loose cement in air is an index of its specific gravity. The increase of strength is shown to be by no means uniform, for, to mention one instance only, cement weighing 110 lbs. per bushel ex-

MR. GRANT'S SUMMARY OF TESTS "SHOWING INCREASE OF STRENGTH WITH INCREASED SPECIFIC GRAVITY."

Age of Samples seven days.

Weight per bushel in lbs....	106	107	108	109	110	111	112	113	114	115	116	117	118
Breaking weight of 2.25 square inches in lbs.....	473	592	650	647	708	694	687	702	700	705	768	718	644
Weight per bushel in lbs.....	119	120	121	122	123	124	125	126	127	128	129	130	
Breaking weight of 2.25 square inches in lbs.....	778	732	706	717	674	820	816	657	865	917	920	914	

ceeded in strength that weighing 126 lbs.

The results of Mr. Colson's numerous experiments on this subject have been epitomised thus:—

Age of Samples seven days.

Weight per bushel in lbs.....	112	113	114	115	116	117	118	119	120	121	122	123	124	125	126	127	
Breaking weight of 2.25 square inches in lbs..	747	688	719	708	729	675	745	706	718	702	732	676	631	545	603	558	

The cement experimented on appears to have been heavier than that which the Author has had an opportunity of testing. The same deduction, however, is derivable from each of the tables, viz., that there is no fixed relation between the weight of cement, as at present ascertained, and its tensile strength.

In dealing with the question of weight, it is necessary to consider the methods of weighing. These are unsatisfactory and open to objections; the ascertained weights bear no definite relation to the real density, and depend, *cæteris paribus*, on the degree of pulverization—finely-ground cement appearing to weigh less per cubic foot than the same cement when coarsely ground, so that, in many cases, difference of weight may simply mean difference in degree of pulverization. With the same cement also the results are not always similar, even when weighed by one person two or three times in succession. Although there does not appear to be much, if any, connection between the weight of cement, as heretofore obtained, and its tensile strength, it would probably be erroneous to assume that the strength is unaffected by the actual density or specific gravity.

It is, therefore, suggested that the present unscientific and comparatively useless operation of weighing should be discarded, and the specific gravity be taken instead. That this has not hitherto been done is probably due to the trouble and difficulty experienced in obtaining the specific gravity by the ordinary method. In the case of Portland cement a liquid must be used which does not chemically affect it. The specific gravity of this liquid has also to be taken and the results reduced to the standard of distilled water, involving tedious arithmetical calculations.

To obviate these difficulties, and to enable the operation to be performed with facility and expedition, the Author has devised an extremely simple gravimeter. It consists of a small glass vessel holding, when filled to a mark on the neck, a given quantity of liquid, and of a glass pipette furnished with a graduated stem and stop-cock, and containing, when filled to a mark on its upper extremity, a volume of liquid equal to that

held by the first-mentioned vessel, *minus* the quantity displaced by 1,000 grains of the densest substance intended to be examined.

In using the gravimeter the pipette is filled to the mark with paraffin, turpentine, spirits of wine, or any other liquid which does not act on the cement, preferably paraffin; 1,000 grains of the cement are then introduced into the smaller vessel, which is placed under the pipette and filled to the mark. Before this is quite completed the vessel may be corked, and the contents shaken to remove any small air-bubbles that may be entangled in the cement. The height of the column of liquid remaining in the pipette determines the specific gravity, which can be at once read off on the graduated stem. It is manifest that the denser the substance operated upon, the less liquid will be displaced in the smaller vessel, and therefore the less will remain in the pipette, and *vice versa*. In reading the accompanying gravimeter, the second place of decimals is estimated. Any greater degree of delicacy may be obtained either by diminishing the diameter of the stem or by reducing the range.

The specific gravity of any solid substance coming within the range of the instrument can, of course, be taken in the same manner. The advantages claimed for this gravimeter are, that neither the density nor the temperature of the liquid used need be taken into account; one weighing is sufficient, and all arithmetical calculations are dispensed with; it is also inexpensive, and requires little skill in manipulation.

In order to test the accuracy of the instrument, a small piece of granite was reduced to powder and its specific gravity taken by the gravimeter; the specific gravity of an unpulverized piece was then ascertained by the ordinary method. Similar experiments were also made with a piece of limestone. The results were :

Granite :
Specific gravity by gravimeter.....2.62
" " " ordinary method.2.63

Limestone :
Specific gravity by gravimeter.....2.70
" " " ordinary method.2.71

Some of the results obtained in comparing the weight of cement of various manufacturers, as usually taken, with the actual density or specific gravity, are shown in Table 2.

TABLE 2.

Weight of 1 cubic foot of Cement, as ordinarily obtained.	Specific Gravity.
lbs.	
75.50	2.91
81.50	2.80
83.25	2.96
84.25	3.03
85.00	2.93
85.00	2.96
85.50	2.82
87.50	2.91
89.00	2.96

The weight therefore, as ordinarily found, bears no relation to the density. Taking the whole number of experiments (about fifty) the specific gravity varied from 2.77 to 3.03, the average being 2.91, showing that Portland cement is heavier than ordinary building stone. The specific gravity of a number of specimens of fine sifted cement gave an average of 2.9; that of the coarse particles of the same cements being 2.93. The density of a specimen of unground clinker was 2.55; the clinker contained numerous small air-holes, which accounts for the specific gravity being less than that of the ground cement. Some samples of gauged cement, the age of which was three months, had an average density of 2.21, the extremes being 2.07 and 2.45.

PULVERIZATION.

The degree of pulverization at present demanded in this country seems to depend upon the judgment of the engineer. In the author's experiments, the fineness of the cement was measured by a sieve with circular perforations, the perforations being 0.0231 inch in diameter. Coarsely-ground cement, when gauged neat, generally possesses greater tensile strength than finely ground cement. On this subject a series of experiments was made by the author, some of the results of which are given in Table 3 (next page).

The "fine sifted" cement was that which passed through a sieve with apertures 0.0231 inch in diameter, the various degrees of coarseness being

TABLE 3.—EFFECT OF COARSENESS ON THE STRENGTH OF CEMENT. SAMPLES GAUGED NEAT.

Breaking Strain per square inch after seven days' immersion.

Fine Sifted Cement.	Percentage of Coarse Particles.				
	5	10	15	20	25
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
355	..	435	..	..	471
439	..	..	..	..	.
476	441	493	477	..	357
313	288	324	323	..	404
403	..	466	406	369	..
471	..	505	..	..	..
333	331	369	412	..	..
443	458	465	..	..	..
396	..	400	..	..	..
392	..	392	386	..	..
257	..	..	..	286	..
445	430	..	437	..	..
..	410	445	458	..	..
371	..	319	435	453	..
394	402	404	429	426	..
486	..	558	560	541	..
462	449	463	463	467	..
183*	212	246	276	270	..
281	259	248	311	254	..

* Very slow-setting.

produced by adding the requisite amount of coarse particles, care being taken to mix them thoroughly with the fine. Each number represents the average breaking weight per square inch of from three to six samples; the age of the samples was seven days, and they were taken promiscuously from the cement of different London manufacturers, and of one French manufacturer.

The experiments of Mr. Colson on this subject confirm those of the author, for with samples one month old, the breaking weight of 2¼ square inches was, for fine screened cement, 939 lbs., and for coarse unscreened, 989 lbs. When the age of the samples was six months, the results were similar.

It appears singular that when the coarse particles are mixed with the fine particles the strength of the sample should be increased, as when gauged by themselves the coarse particles do not evince the slightest tendency to bond together or set, but remain loose and inert like so much sand. This result, however, would seem to be in accordance with what occurs in the case of limes. Admitting Rondelet's statement, as to the adhesion of mortar to stone exceeding its own tensile strength, to be true also of cement, then as the tensile strength of the unground particle *per se* may be assumed to exceed that of fine cement recently gauged, it follows that within certain limits the presence of the coarse particles, which may be conceived to act as minute stones, increases the strength of the aggregate above that of its matrix.

In the analogous case of hydraulic limes the strength is similarly increased by the admixture of clean sand. The coarse particles of cement, although probably acting somewhat in the same way as the sand, seem to present additional facilities for the adherence of the fine cement to them. This may be seen from the following experiments, in which the cement of four different manufacturers was used, and a series of samples made from each; the grains of sand were somewhat larger than the coarse particles of the cement. The number of experiments is limited, but the results are marked.

TABLE 4.—EFFECT OF COARSE PARTICLES OF CEMENT AS COMPARED WITH SAND.

Fine sifted Cement and Sand in equal parts by Weight.			Fine sifted Cement and coarse particles in equal parts by Weight.		
Area.	Breaking weight.	Breaki'g Weight per Sq. Inch.	Area.	Breaking weight.	Breaki'g Weight per Sq. Inch.
Sq. in.	lbs.	lbs.	Sq. in.	lbs.	lbs.
2.26	472	209	2.27	510	224
2.25	452	200	2.26	402	178
2.25	360	160	2.24	517	230
2.28	530	232	2.27	850	374
2.32	522	225	2.30	965	419
2.32	567	244	2.34	945	404
2.32	400	172	2.27	662	291
2.32	422	182	2.36	622	263
2.30	422	183	2.25	600	266
2.22	580	261	2.28	963	422
2.25	592	263	2.30	937	407
2.25	610	271	2.28	1,060	465
Average...		217	Average...		328

The author has frequently found, in the case of cement containing not more than 10 per cent. of coarse particles, that they may be replaced by sand without affecting the strength of the aggregate; beyond 10 per cent. the results were increasingly in favor of the

coarse particles as compared with sand. In some instances, increasing the quantity of coarse cement from 10 to 100 per cent. only diminished the strength by about 12 per cent.

In order to ascertain the effect of coarsely-ground cement on mortar, experiments were made with cement, of various degrees of coarseness, and sand gauged in equal volumes; the mean results are given in Table 5. As before, the cement of different manufacturers was used, and each of the breaking weights given is the average of not less than three careful tests. From this table it would appear that, when diluted with an equal volume of sand, coarse cement does not give quite such favorable results as fine. From various considerations it seems probable that, in the proportion of 1 part of cement to 1 of sand, the limit is reached, if not exceeded, in which the coarse cement is as effective as the fine, and that beyond this limit the coarse particles act nearly the same as so much additional sand.

TABLE 5.—EFFECT OF COARSENESS ON THE STRENGTH OF CEMENT.

Samples gauged with Cement and Sand in equal volumes. Breaking strain after seven days' immersion per square inch.

Fine sifted + Sand.	Percentage of coarse Cement.		
	10 + Sand.	15 + Sand.	20 + Sand.
lbs.	lbs.	lbs.	lbs.
208	224	..	..
241	..	235	..
268	277	248	..
272	249	..	229
170	203	..	199
233	251	230	..
272	230	274	294
210	206	205	208
233	215	187	..
220	230	252	265
257	251	275	295
61*	150	98	103
175	187	179	176

* Twenty-four hours in setting.

In many instances the author has found that mortar consisting of 1 part of cement to 3 parts of sand,—the cement containing 25 per cent. of coarse particles,—possessed little more than one-half the tensile strength of mortar gauged in the same proportions with fine sifted cement, the age of the samples being four weeks.

Mr. Colson's experiments lead to the same conclusion, and show that, gauged in the proportion of 1 of cement to 1 of sand, there was only a slight difference in favor of the fine cement; but that when gauged in the proportion of 1 of cement to 2 of sand, the coarseness of the cement had the effect of diminishing the strength to the extent of about 14 per cent. There can be no doubt that the diminution in the efficacy of the coarse cement will be more apparent as the proportion of sand is increased.

In the investigation of this part of the subject, which may be termed the mechanical agency of the cement particles, it is of the highest importance to understand, if possible, by what means the cementing material binds substances together. The explanation given by the French chemist Macquer, of the analogous action of limes, is probably equally applicable to Portland cement. This is to the effect that the extreme fineness of the particles of lime which reduces it altogether to surfaces, gives it the faculty of adhering firmly to the surface of the sand, or stone, and with a force proportioned to the closeness of the contact. This explanation omits the effect produced by crystallization, which probably acts an important part, the setting of cement being attributable to the formation of hydrated silicates and aluminates, such crystals adhering strongly to foreign substances; and as the more minute the division of the particles, the more perfect will be the subsequent crystallization, an additional argument is presented in favor of finely-ground cement.

The extra cost of grinding cement, to the almost impalpable powder which theory would indicate, can hardly be compensated by the increased efficacy thereby obtained; but it is extremely desirable, and comparatively easy, to discover the limit at which extra grinding ceases to be economical. On the whole, the author believes that cement, of which not more than 10 per cent. is stopped by a sieve with perforations $\frac{1}{50}$ inch in diameter, probably approaches the present economical limit.

In connection with the subject of mechanical agency, the author has made

some experiments on the effect produced by sands, of different degrees of granulation, on the strength of cement mortar. Some of the final results, as representing the average of a rather disconnected and tentative series of tests, are given in Table 6. In each case similar sand and cement were used, the various degrees of fineness being obtained by repeated sifting.

TABLE 6.—EFFECT PRODUCED BY FINENESS OF SAND ON STRENGTH OF MORTAR, 2 PARTS OF SAND TO 1 PART CEMENT BY MEASURE.

Age of Samples four weeks.

Sieves Nos. 1 and 2. Sand passing No. 1 but stopped by No. 2.			Sieves Nos. 2 and 3. Sand passing No. 2 but stopped by No. 3.			Sieves Nos. 3 and 4. Sand passing No. 3 but stopped by No. 4.			Sieves Nos. 4 and 5. Sand passing No. 4 but stopped by No. 5.			Sieves Nos. 5 & 6. Sand passing No. 5 but stopped by No. 6.		
Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.
Sq. ins	lbs.	lbs.	Sq. ins	lbs.	lbs.	Sq. ins	lbs.	lbs.	Sq. ins	lbs.	lbs.	Sq. ins	lbs.	lbs.
2.22	375	169	2.20	292	133	2.34	279	119	2.26	200	88	2.22	198	89
2.22	375	169	2.30	295	129	2.30	270	117	2.22	230	103	2.21	170	77
2.18	375	172	2.32	285	122	2.40	292	121	2.21	220	99	2.21	185	83
2.18	375	172	2.32	287	123	2.25	270	120	2.24	240	107	2.23	210	94
2.22	398	179	2.22	278	125	2.22	284	128	2.24	245	109	2.24	103	46
2.22	396	178	2.22	295	133	..	..	..	2.25	270	120	2.21	135	61
2.25	406	180	..	..	..	..	..	..	..	..	..	2.20	219	99
2.17	456	210	..	..	..	..	..	..	..	..	..	2.25	210	93
2.17	452	208	..	..	..	..	..	..	..	..	..	2.17	208	95
2.23	442	198	..	..	..	..	..	..	..	..	..	2.26	214	94
2.17	363	167	..	..	..	..	..	..	..	..	..	2.20	216	98
2.17	418	192	..	..	..	..	..	..	..	..	..	2.26	216	95
Average..		183	Average..		127	Average..		121	Average..		104	Average.		85

DETAILS OF SIEVES.

			Inch.	
No.	1	circular perforations	0.14	in diameter
"	2	" "	0.10	"
"	3	" "	0.07	"
"	4	" "	0.02	"
"	5	wire gauze	2,900 meshes to a square inch.	
"	6	"	8,200 "	"

The sand passing through No. 1 and rejected by No. 2 was considerably coarser than ordinary building sand. That passing No. 5 and stopped by No. 6 was of a degree of fineness frequently met with in practice, and known as 'woolly' or 'running' sand.

TENSILE STRENGTH.

As before observed, the tensile strength is the test to which all the others must be subservient. In practice cement has to resist both tension and compression; but its power to resist the latter is so closely allied to the resistance it offers to the former, as to render the test of tensile strength sufficient. In fact, it would appear that this test alone, if properly applied, not only to the cement in its neat condition, but also when diluted with sand, would answer all practical purposes. It is proposed, therefore, to treat this part of the subject in detail, though it may unavoidably lead to some repetition of what is already known.

METHOD OF MINING THE SAMPLE BRIQUETTE.

This determines to a great extent the breaking weight. Everything that conduces to uniformity should be observed, and the mould should be carefully filled with gauged cement, so as to leave no air-holes or other defects.

The following method, adopted by the

Author, gave good results*:—after being thoroughly mixed and incorporated by a light (plasterer's) trowel, the cement was packed closely into the mould, heaped a little higher than the edges, and pressure applied by means of a small board held in the hands of the operator; this may be done a second time, after adding a little dryly gauged cement. The pressure forces out any superfluous water, replaces it with cement, and prevents the occurrence of air-holes or other defects.

The application of pressure in the formation of the sample is not inconsistent, for in practice the cement, forming the joints of masonry or the matrix in concrete, is subject to much greater pressure by the weight of the superincumbent work. From a large number of experiments the Author found that the slight pressure referred to increased the strength of the sample more than twenty-five per cent. This result is satisfactory, as it shows that the pressure unavoidable in practice is probably beneficial. It is desirable to adopt every means to obtain the best results, and to develope as much as possible the strength of the cement under test. Manufacturers are liable to suffer some injustice from the want of proper care and skill in the making of test blocks, for two sets of samples of the same cement, presenting the same external appearance, can be made to give very different results, the strength of one being nearly double that of the other at the end of seven days.

PROPORTION OF WATER USED IN GAUGING.

The least possible quantity of water should be used in gauging, any excess having a direct tendency to reduce the strength of the sample. For this reason Portland cement should not be tested while too fresh. If warm, it should be spread out and allowed to cool for a few days, and then be made into test blocks. The cooling process—by which the particles of lime are slowly slaked by the absorption of water from the air—has frequently produced an improvement in the strength of seven-day samples of about forty per cent.

The following table gives some of the results obtained by using various proportions of water:

TABLE 7.—CEMENT GAUGED WITH VARIOUS PROPORTIONS OF WATER.

Age of Sample seven days. Average breaking weight per square inch.

	5 oz. Water to 32 oz. Cement.	6 oz. Water to 32 oz. Cement.	7 oz. Water to 32 oz. Cement.	8 oz. Water to 32 oz. Cement.	9 oz. Water to 32 oz. Cement.	10 oz. Water to 32 oz. Cement.
	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
	433	460	..	306	..	..
	416	435	..	296	..	..
	416	480	..	282	..	..
	344	398	382	363	223	184
	368	366	387	357	219	221
	329	416	353	339	211	197
	487	489	323	316	187	163
	474	447	420	302	234	165
	440	471	336	268	209	162
Averages ...	412	440	367	314	214	182

With 5 ounces of water to 32 ounces of cement the gauged cement was extremely dry, crumbling under the trowel, and could not be made to take a smooth surface; with the proportion of 6 ounces to 32 ounces it was moderately dry, and could be finished and smoothed off with the trowel; with 7 ounces to 32 ounces the mass was moderately wet; with eight ounces to 32 ounces the samples were wet and soft; with 9 ounces to 32 ounces they had the consistency of stiff grout; and with 10 ounces to 32 ounces a liquid was produced which could be poured from one vessel to another. In the last two cases the samples shrank consider-

* Except in the case of very slow-setting cement, only so much was gauged at a time as sufficed to make one sample, the water for gauging being added at one operation.

ably, and must have lost some of the water by evaporation.

The time occupied in setting increased, in proportion to the quantity of water used, from fifteen minutes to about forty-eight hours. The effect of using an excessive quantity of water is manifest in seven-day samples, but it has been questioned whether the deterioration is permanent. Mr. Colson's experiments go to prove that the deterioration continues, the decrease of strength being more apparent in sample six months old than in others only seven days old. The best test blocks were made with from 6 ounces to 7 ounces of water to 32 ounces of cement; with this proportion the gauged mass presented a somewhat dry and granular appearance, but when placed in the mould and subjected to the small pressure before referred to, a little water exuded from under the mould, and the upper surface became soft and capable of being neatly smoothed off with the trowel.

SHAPE OF THE SAMPLE BRIQUETTE.

The form usually adopted (Fig. 1) appears capable of improvement. In the

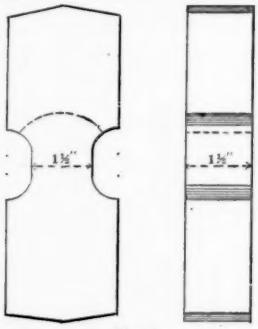


Fig. 1.

large majority of cases the line of fracture is not at the point of minimum section, but generally somewhat as shown by the dotted line in the left hand figure. This result has been attributed to a peculiar effect produced by change of form on the strength of materials. The Author, however, believes it to be chiefly due to irregular strains produced by the clips holding the sample, which clips, having a tendency to collapse, lead to crushing strains. That some irregular strain of this kind takes place seems probable from the fact, that weak samples ordinarily break at the point of minimum section, while samples of greater strength, requiring the application of heavy strains, generally break through the thicker portion, the latter result apparently arising when the strain on the clips is sufficient to cause collapse. Differences also frequently occur in the breaking weights of samples of similar make and from the same cement.

In order to avoid the use of clips, the Author had holes drilled in the samples and the narrow part reduced in area (Fig. 2); this reduction was made with a view to prevent fracture through the eye; they were then broken by steel pins passing through the eyes. In this way an increase in the breaking weight of more than twelve per cent. was obtained, the samples in every case but one breaking through the smallest section.

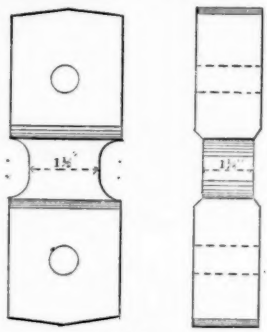


Fig. 2.

Some of these experiments are given in Table 8. The samples on each horizontal line were made of the same cement, at the same time, and were gauged with a similar quantity of water.

Subsequently samples were made of the shape shown in Fig. 3, with which very satisfactory results were obtained; and it is believed that this, or some similar, shape will be found superior to the form now generally in use.

In Table 9 some of the results of experiments made with the new-shaped sample are given. This table shows an increase of nearly twenty per cent. in favor of the proposed shape. In a few instances the samples broke through the eye, but this is attributable to the temporary character of the change in the

TABLE 8.—COMPARISON BETWEEN ORDINARY SHAPED SAMPLES BROKEN WITH CLIPS AND BROKEN WITH STEEL PINS PASSED THROUGH DRILLED HOLES.

Age seven days.

Ordinary Shape. Broken with Clips.			Ordinary Shape. Broken with Steel Pins through Drilled Eyes.		
Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.
Sq ins	lbs.	lbs.	Sq. ins.	lbs.	lbs.
2.32	790	340	1.800	830	461
2.32	875	377	1.650	755	457
2.36	845	368	1.800	663	368
2.32	884	380	1.830	832	454
2.34	951	406	1.830	834	455
2.30	967	420	1.800	773	429
2.30	910	395	1.545	695	449
2.32	910	385	1.635	800	489
2.36	999	423	1.635	825	504
2.25	999	444	1.570	776	494
2.32	1,087	468	1.680	924	550
2.30	1,026	446	1.650	645	391*
2.34	1,030	440	1.650	773	468
Average..		407	Average....		458

* Badly drilled.

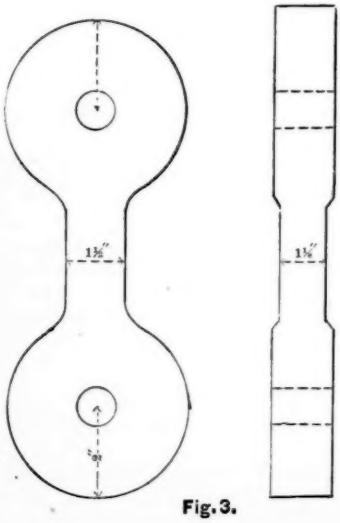


Fig. 3.

TABLE 9.—COMPARISON BETWEEN SAMPLES OF ORDINARY SHAPE BROKEN WITH CLIPS, AND OF AN IMPROVED SHAPE BROKEN WITH STEEL PINS PASSED THROUGH EYES MOULDED IN THE CIRCULAR ENDS OF THE SAMPLE.

Age seven days.

Broken with Clips. Ordinary Shape.			Broken with Steel Pins. Improved Shape.		
Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.
Sq. ins	lbs.	lbs.	Sq. ins.	lbs.	lbs.
2.27	840	370	2.36	830	351
2.36	790	335	2.36	997	422
2.34	750	320	2.37	820	346
2.24	520	232	1.77	460	261
2.27	645	283	2.30	915	398*
2.40	880	366	2.45	1,120	457*
2.30	655	284	2.36	890	377*
2.25	637	283	2.34	790	337
2.23	590	264	2.28	712	312
2.26	545	241	2.32	717	309
Average...		297	Average....		357

* Broke through eye.

testing-machine to adapt it to the altered samples, great care and accuracy being necessary, in order that the strain shall pass directly through the axis of the sample. The form suggested is similar to that generally used in testing the tensile strength of iron, and there does not appear to be any reason why what has been found most suitable for iron should not also apply to Portland cement. In this case also the samples on each horizontal line were under like conditions as to the cement, time, and quantity of water.

AGE AT WHICH THE SAMPLE BRIQUETTE SHOULD BE BROKEN.

The author has confined his observations almost exclusively to seven-day samples, as that period seems to be universally adopted; but it may be questioned whether this age gives a fair test in all cases. If the cement is moderately quick-setting, becoming sufficiently hard to be removed from the moulds in from half an hour to five hours after gauging, the seven-day test is probably a fair exponent of the quality; with such cement,

taking the average of several hundred experiments, the breaking weight of the cement at seven days was nearly always in direct relation to the breaking weight at twenty-eight days. Warm cement can be cooled by exposure, until the time of setting comes within the limits mentioned. In the case, however, of very slow-setting cement, requiring from six to twenty-four hours to harden, the breaking weight at seven days does not seem to bear the same ratio to the ultimate strength as in the quicker setting cement, the latter gaining strength more rapidly, although it may ultimately be inferior to that of the former.

The longer the time between the gauging and breaking of the samples, the more reliable are the results; but whether the advantage thus gained is a compensation for the inconvenience arising from the lengthening of the time seems doubtful. Generally, the cement, as received from the manufacturer, sets with sufficient quickness to enable its quality to be determined by the seven-day test. The difference in the strength of seven and twenty-eight day samples was found, from numerous experiments, to be about 20 per cent.

It might be desirable to introduce along with the ordinary seven-day test one including a longer period, say twelve weeks, so that if the first should appear unsatisfactory, which might occur in the case of very slow-setting cement, the latter could be tried before the cement was finally rejected.

It has been sugested that cement mortar should be made the subject of test, as it is in this form that Portland cement is usually employed in practice. If, however, the properties of cement are understood, and its behaviour when diluted with sand, &c., a test of this description is unnecessary.

STANDARD OF TENSILE STRENGTH.

The standard test, specified at the commencement of the London Main Drainage works, was a breaking weight of 400 lbs. on an area of 2.25 square inches (equal to about 178 lbs. per square inch) after seven day's immersion. At present a tensile strength of 350 lbs. per square inch is frequently demanded and obtained, and it seems probable that the standard may be raised still higher, as the manufacture of the article improves and its properties become better understood.

In the author's experiments, in which the moulds and testing-machine were those in ordinary use, it was not uncommon to obtain a strength of from 400 lbs. to 500 lbs. per square inch after seven days's immersion, and in one or two instances it rose as high as 600 lbs. per square inch. The average of a large number of tests made within the last two or three years has been 380 lbs. after seven days, and 450 lbs. per square inch for twenty-eight-day samples. The fineness of the cement was such that about 7 per cent. by weight was stopped by a sieve the perforations in which were about $\frac{1}{43}$ inch in diameter. Having due regard to the effect of fine grinding on the strength of neat cement, and to the rates at which the strength of different samples increases in the early periods of their age, the author believes that a minimum breaking weight of from 350 to 370 lbs. per square inch is as much as can be reasonably expected from seven-day samples.

In conclusion, the author would observe that the experiments he has been enabled to record are but limited, considering the nature of the subject dealt with. They were, however, made with great care, under his own direct supervision, most of them being spread over the last three or four years.

FORGING STEEL.—We would here merely direct attention to a small memoir that has recently appeared, entitled "Remarks on the Manufacture of Steel and the Mode of Working it," by D. Chernoff, assistant-manager of the well-known Abouchoff Steel Works, near St. Petersburg, in Russia, translated by Mr. W. Anderson, M.I.C.E., and published by W. Clowes and Sons, in London. It occupies itself principally with the forging of steel and the means of preventing a crystalline structure developing itself in the mass, which is done by a regulation of the temperature. We must, however, refer to the original for details. This paper has also been reproduced in the *Engineer* and in *Engineering*.

THE USE OF MACHINERY IN ITS RELATION TO ARTISTIC PRODUCTIONS.

By WILLIAM BRAGGE, F.S.A.

From "The Builder."

Last year Mr. Poynter, the President of the Art section of the Social Science Congress, held at Liverpool, in his address to the Congress, objected to the "Castellani" collection of Italian jewelry being sent to Birmingham on loan, because cheap and inferior copies would there be made mechanically, and in this objection Mr. Poynter embodied and expressed a very common idea,—that mechanical work cannot be artistic work.

How far this is true I shall endeavor briefly to show, and I may at once state that I shall claim for all mechanical appliances the right and privilege of assisting in our art manufactures. I am prepared at once to admit that the enormous industrial progress of this country during the present century has really done very little for art. Our manufacturers have, almost to the present time, given their whole attention to economy and rapidity of production, and the public taste has not been elevated by the style of the articles produced. But allowing that in past and even at the present time, the taste and skill of the artist have been lost,—sacrificed to the ingenuity and contrivances of the mechanican,—we surely need not conclude that art cannot become an ordinary adjunct to mechanical reproduction. We know well that in many manufactures carried on by us to-day our best models in design, as well as in material, are those bequeathed to us by workers who lived centuries ago, whose mechanical appliances were of simplest and rudest character; and it is with some sense of humiliation that we are bound to confess ourselves unable to equal not merely the works of three or four centuries ago of European civilisation, but even of those of the present day, produced by nations which we are conceited enough to call savage. Let us take a few examples of the arts and manufactures of old time and compare them with those of the present day. Have we improved in the art of printing since its invention, 400 years ago? or in the art of paper-making of 600 years ago? In quality, certainly not; we are bound to remember that to-day an ordinary edition of a newspaper is printed on both sides in quantity sufficient to cover an acre, in the same or less time than Caxton or Guttenberg required for the production of a single sheet. We produce paper now in continuous rolls of many hundreds of yards in length, and with a rapidity commensurate with the voracious requirements of the steam printing-press; but no paper now in general use (excepting bank-note paper) equals in quality that upon which our early books are printed. Most of the books printed of late are, I think, likely to be entirely lost within a century, simply from the natural decay of the (so-called) paper upon which they are printed. But we can to-day buy a newspaper for a penny or a book for a shilling, which in Caxton's time would have cost fifty or a hundred times as much. We can and do engrave dies and coin money for all the world, but we cannot produce anything comparable with the exquisite coins of ancient Greece. We can imitate in a feeble way the cameos and intaglios of the stone engravers of ancient Rome; but this is an art almost unpractised amongst us, and so far as we are concerned artistically, it is practically lost. In ornamental enamel we must go to China and Japan for our models, as Messrs. Elkington have wisely done, and then find out by painful experience how best to copy the humbler objects, before daring to imitate the more important. Our English enamelling of to-day is confined pretty closely to the decoration of mayor's chains, Masonic emblems, and the lining of iron pots and pans. The art of lacemaking, or ornamental needlework —in Mediæval times one of the most general occupations of women of every rank above the lowest,—is now almost entirely lost, and we must go to our museums to see the charming examples of taste in design and perfection in work which still remains for us to admire.

The hand-made lace will bear microscopic examination, consumed much time and patience in its production, and was consequently very costly. Now-a-days one can buy in Nottingham embroidered curtains enough to furnish a house for less money than a collar of ancient point lace would cost. I might indefinitely prolong this list, and point out to you the comparatively lost arts of damasquining, or inlaying iron and steel with the precious metals; of the special forging of sword-blades in Damascus and Toledo to produce the twist in the fiber, so to speak of the metal; of the remarkable skill in perforating steel and brass as a decoration of useful objects practised in Persia; of the marvels of design, color, and glaze of early Italian pottery or majolica; of the exquisite glass and mosaics of Venice; the gold and silversmith's work of Italy; the shawls of Cashmere, and textile fabrics generally of India; but further instances are not needed, and you will, I think, agree with that which is a commonly accepted opinion, that in all these, and in many other artistic, manufactures, we, the English, are now utterly unable to compete. I have not made in the foregoing remarks any reference to painting and sculpture, because these arts stand outside of the reach of mechanical appliances, and therefore the painter and sculptor of to-day are in this respect in the same position as were Apelles and Phidias, Titian and Michelangelo. And now we may proceed to consider what is the real or supposed antagonism between art as developed in individual workmanship, and as accompanied or assisted, by mechanical appliances. If we fairly compare the art of, say, the goldsmith or silversmith of three centuries ago with the art of to-day, we find that the workman then lived and worked under influences and incitements which have long ceased to exist. The Mediæval workman thought and cared only for the single object upon which he was employed. He threw his whole soul, his whole inventive faculty, his whole technical skill, into his darling work, and he knew that no meaner hand could rob him of the fruits of his patient labor and skill. He was not hurried with his work. He had no anxieties as to cost. His master, if he had one, was not perpetually crying out for the economies of labor and of material, no estimates had been given which could not be exceeded, and no arbitrary percentage of profit had been beforehand fixed. Is it extraordinary that under such favorable conditions the workman should produce his best? Certainly not, and we ought to be profoundly thankful that the results of such a state of things have been preserved to us for our special instruction. How does the working gold or silver smith of to-day stand in relation to his work as compared with his predecessor of three centuries ago? Truly in a miserable plight! Instead of being himself the creator of the design he has to carry out, it has probably been prepared for him in an office where the commercial economy of manufacture is more considered than the principles of art, where facility of production is held to be more important than elegance of form, and where the demands of the Demon of Fashion override the desires of good taste. All the patient labor of hammer and chisel, slowly and surely giving expression to the taste of the individual, are dispensed with now, and are replaced by a few blows of the stamping-machine! This stamping-machine or press has taken the place of hand labor, and being only a machine, it is perfectly indifferent whether the work it is called upon to do is in good or in bad taste. It is as willing to work for the demon of ugliness as for the spirits of beauty, and the work which it produces is simply a reflex of the mind, and taste, and quality of its employer. It is unfair to charge the mechanical appliances of manufacture with having caused a degradation of taste. The degraded taste existed when machines were first employed, and, unfortunately the process of refining and improving the artistic feeling of the public has been hindered by the cheap and ugly mechanical productions. If the modeler will only produce moulds worthy of admiration, the stamping-press is at his command to bring under the daily notice of every one the forms of beauty which he has himself designed. We cannot, if we would, go back to Mediæval habits of thought or modes of work, and those who to-day would abolish all mechanism in art-manufacture would injure instead of helping their cause. The patience and skill of the modeler and

chaser who wishes to devote months or years to the production of a master-piece can still be given. He remains free to use his time and talents as he thinks best, and happily there is yet a demand for works of a high class in the production of which such men may be congenially employed. In this wealthy and art-loving country there is abundant occupation for the most skilful artists. Nothing that is really good fails to find a ready purchaser, and talent has to-day as fair a field before it as it ever had in days gone by. The gifts of corporate or public bodies to men who deserve honor,—the production of prizes for excellence in exhibitions, for racing, or boating, or yachting; the demand of the wealthy for specialties, designed for themselves, ensure for the conscientious workman full scope for his talents. And I am sorry to be compelled to think that this special work is more than enough to occupy all the competent workmen who can be found. In the production of such works machinery and mechanical reproduction have no part, they stand as individual works, each wrought out by a master mind, to fulfil a single purpose, and to satisfy a personal want. But can the requirements of the million be dealt with in this aristocratic manner? Certainly not. For their wants every element of economy must be utilised, the means of production must be assisted by all the resources of mechanical science. The absolute perfection of detail must of necessity be given up, but there is absolutely no reason why a high standard of excellence should not be preserved. Our jewelry, our gas-fittings, our table services, whether of silver, silver-plate, or other metals; our trays, coal-scuttles, fenders, and grates, and everything else which forms a part of our domestic surrounding, may as cheaply be made elegant as ugly. The use of machinery has only to be wisely and thoughtfully adapted, governed by artistic principles, and its result will be to give to the world in its cheapest form the useful and the beautiful. To the working out of this desirable end, I commend you, the students of the Sheffield School of Art.

THE PRESENT STATE OF INDUSTRIAL ART.*

By CHARLES L. EASTLAKE.

From "The Architect."

We have been in the habit for two or three generations past of dividing art into several sections, and of giving them grand names. There is High Art, for instance, and Fine Art, and Pictorial Art, and Textile Art, and Ceramic Art, and Industrial Art and what not, and of these some have been foolishly over-rated on sentimental grounds, and others have been foolishly under-rated on ignorant grounds; and the most curious fact is that all these fine names were invented or adopted during a period when taste in all art had sunk in this country to the lowest standard which perhaps it has ever reached since civilization began. I mean in the early part of the present century. Of course, in saying this, I except such noble instances of artistic power as Turner, Flaxman, and others whose genius shone like stars in the surrounding darkness, but the full excellence of even their work has only lately been appreciated as it should be, while the minor arts, as we are pleased to call them, were clean forgotten in those days or regarded as mechanical and unimportant. Amidst all the tall talk about the grand schools of painting and sculpture, amidst all the foggy theories and pompous dissertations which were imported from Germany about this and that style, and in spite of dilettante clubs and learned societies the arts of manufacture sank lower and lower in character of design, and as a matter of consequence in quality of execution. For you may take it as a fact that speaking generally the worst taste in manufacture is generally found in articles of flimsy workmanship, and the best taste in those of sound honest handicraft, all the world over.

* A Lecture read before the Spitalfields School of Design on March 22, 1877.

The truth is, that while connoisseurs were theorizing about the old masters, while *virtuosi* were examining Greek vases and puzzling their heads to fix the precise period at which each was made, it never seems to have occurred to them that their time might have been better employed in encouraging the art of their own day.

Now don't let me be misunderstood on this point. No one has a greater reverence and admiration for ancient art than I have, and I only wish that all who hear me at this moment could spend their Saturday afternoons alternately at the National Gallery and the South Kensington Museum, both of which contain priceless treasures that cannot be examined too often or too carefully; but do remember this, that no centuries of time can ever add to the merit or detract from the excellence of those works. Those which are admirable now are to be admired, not because of their age or associations, however much our interest may be enhanced on that score—but on account of the skill displayed in their design and execution—just as they stood on the painter's easel, or were displayed in the potter's shop, or were carried away from the blacksmith's forge. There were degrees of excellence then as now, and of course some failures, and when we see and recognize them, whether in a picture frame or a cabinet, we should have the courage to say so, whatever their antiquity may be.

Now the connoisseurs of our grandfather's time talked a vast deal about the Classic school of landscape-painting, about composition and chiaroscuro, and Heaven knows what besides, but they let two of the greatest landscape painters the world has seen—William Turner and David Cox—work for years without finding out their merits, and I could mention others who, at the outset of their career, had to encounter similar difficulties.

The moral which we may draw from all this is, I fancy, plain enough. If a genuine love of art exists at anytime it will be found to extend forwards as well as backwards—in other words, it will encourage excellence in contemporary work and not content itself with hoarding up examples of a bygone age for the mere sake of their rarity, or what I fear is too frequently the case for the sake of their market value.

Further: I may say that a genuine love of art, whether in a nation or as an individual characteristic, should represent something more than the wealth of a museum or the pride of a collector. It should reach and invest with interest the home of every householder, however humble, throughout the land. One of the greatest historians of our time, Lord Macaulay, in the first pages of his greatest work, made scornful allusion to that absurd phrase—"the dignity of History," and pointed out that a history which did not include some notice of the domestic life of our ancestors would be no history at all. Now I think there is an equally absurd phrase, which is of recent origin and which is equally open to ridicule, and that is the "dignity of art." That expression, as commonly understood, means this—that art is a luxury which can only be enjoyed by the wealthy and highly cultivated; that it is the special property of the collector or the *savant;* that those who cannot afford to buy expensive pictures or rare specimens of bric-a-brac must be content with ugly houses and commonplace furniture. This melancholy and selfish creed is, I hope, dying out in our country; but depend upon this, that while a vestige of it remains, there will be little chance of improvement in really national taste. It is precisely because art has been looked on in this light, and talked of as something beyond the reach and appreciation of the people, that the people have grown indifferent to it, and have come to regard it as a thing of the past, the traditions of which are preserved by the world of fashion like armorial bearings or a footman's livery.

Now of course it is very difficult for us in these days to ascertain how far the working classes of any nation in by-gone times enjoyed the highest and most refined achievements of art; but one thing is certain, that down to the end of the seventeenth century the difference between what we now call "high art" and the ordinary arts of manufacture was only a difference of degree and quality—not one of style. The houses of the wealthy in those days were, of course, furnished with articles of costly material and elaborate workmanship, which were

beyond the reach of humbler citizens, but the mode and manner of their execution was the same throughout the land, and the consistency of taste pervaded all the arts, from architecture down to the mere fashion of dress. How the change came about which we recognize in the present day; how the line came to be drawn between high art in some quarters, and no art at all in others, is a very complex question, which, to investigate thoroughly, would involve considerations beyond the scope of this lecture. It may, however, be well to point out that among the causes which have led to this result are the progress of education, and the rapid increase of wealth, which have taken place within the last century.

I am afraid this will sound rather startling to many of you until I explain my meaning, but there is really no cause for alarm, for the social progress of a nation is of infinitely more importance than the improvement of public taste; and if one has interfered with the other for awhile it only remains for the present generation to devise some means by which these two objects can be attained at one and the same time.

In former ages, as you know, the habits of the working-classes were far simpler and more domesticated than they are at the present day. Men, who in country towns and villages plied their trade as joiners, weavers, blacksmiths, or what not, were content to work as their fathers and grandfathers did before them, and generally brought up one or more of their sons to the same business. In this way the traditions of their craft, whether in regard to design, technical knowledge of material, or method of work were preserved, and as all these improved with experience, you may suppose that the result was most beneficial to every branch of industrial art. But by and by many changes came about. Railroads were laid down; schools were set up; cheap newspapers were circulated; some of these honest folks began to get ambitious; they traveled; they read the papers; they sent their boys to school. All this cost money, and the consequence was they were obliged to do more work in the same time. Of course the work was hurried, and not so well done. The sons of these men, being better educated than their fathers, began to grow conceited, and look to down upon their fathers' work. Sometimes they went off to "better themselves," as the phrase is, and were replaced by journeymen, who being paid by the day, and having no interest in the business, naturally thought more of their wages than their work, and moved from place to place whenever they saw a prospect of higher pay, leaving the work to fresh and inexperienced hands. Nor was this all. As work went on increasing, for the sake of wage, population went on increasing too, and unfortunately at a far greater rate than money. Provisions, clothes, and many of the essentials of life became dearer. More work became necessary to pay for them, and at length masters themselves began to consider how they could execute the same amount of work with less trouble, or a greater amount of work at a cheaper rate.

The result of all this, as you may suppose, was to substitute an inferior class of handicraft and material. Cast iron took the place of wrought. In joinery, mouldings, instead of being worked in the solid, were run out by machinery and planted on. Common druggets were printed in imitation of woven carpets. Brickwork was "scamped," and plastered over with stucco or cement in imitation of stone. Deal was grained in imitation of oak or marble. Crockery, which had been formerly painted by hand, was decorated with printed patterns. Much of this work and many of these articles being of course cheaper than what had formerly been produced, suited the pockets of pretentious people, who like to make a show at a small cost; and the consequence was a gradual increase of demand for what I will venture to call cheap and nasty work, and a neglect of those honest trades which were once the pride of the British workman.

The traditions of some few, indeed, have survived, probably because no shams could possibly be practised in them without at once rendering the objects produced useless for their purpose. To this day I know no specimen of native manufacture more satisfactory in construction and more picturesque in appearance than a rustic cart. It is always solidly made of stout timber and well hammered iron. Such little graces of decoration as its purpose admits, in

the way of turned woodwork, chamfered edges, or applied color, are simple and unaffected. It serves its object well and honestly, and has filled the page of many an artist's sketch book. And yet the maker of that cart may perhaps have never learned to read or write, and knows no more of industrial art than he does of the Differential Calculus. That is the kind of work which we should try and revive in this country; that is the kind of work which no school of design can improve.

Pray don't suppose that I am putting an extreme case, or that I wish to say a word in disparagement of this and many other excellent institutions which have become a necessity at the present time. All I desire to point out is that their object should be to start from the principles which guided our forefathers in design and construction. When we have mastered those principles and learned to work well and thoroughly in any one style of a bygone age, it will be time enough to think of inventing a new style, about which ignorant people often talk, and with as much reason as if they proposed to invent a new language.

Take, for instance, the noble decorative art of Japan as it has been applied for centuries past to the design of pottery, of lacquered work, and of textile fabrics. That is something like industrial art, if you will, and what is the secret of its excellence? It is traditional work handed down from generation to generation, with infinite varieties of form and color it is true, but with no change of style in the ordinary acceptation of the word.

Now, mark what has happened. The progress of civilisation goes on. Commercial enterprise and increased facilities of travel afford opportunities of intercommunication between Europe and the East. Japanese goods are brought to the English market, and specimens of British manufacture go to Japan. All very satisfactory in the interests of trade no doubt, but meanwhile the Japanese artist, after examining the work produced by this great and enlightened English nation, and perhaps getting a hint or two from our merchants, comes to the conclusion that he had better modify his taste to suit our notions of elegance, and the result for years past has been a gradual deterioration of Japanese art.

This is a melancholy state of things, for which it would be difficult, if not impossible, to devise a remedy, and if I dwell upon it, it is to show you the fallacy of supposing, as many excellent and well-meaning persons do, that the progress of good taste in manufacture must necessarily go hand in hand with what used to be called the march of intellect. The very fact that we have learnt so much, that we have brought before us so constantly at international exhibitions, and by means of published works, specimens of the art of other nations, puzzles the designer as to choice of style, and makes him borrow a hint from this and a trick from that school of taste. Conditions of form and schemes of color, which properly belong to one branch of manufacture, are too often imported into another, and we may congratulate ourselves if the result is not a hopeless jumble of ill-assorted materials, misapplied ornament, and inappropriate construction.

How can we escape from these difficultles? It is easy to say, "Go to the South Kensington Museum and study the lent examples of ancient art." But I cannot fancy a more bewildering task for the student—I am speaking of beginners of course—than to wander through that palace of countless treasures, including specimens—good, bad, and indifferent—of every age and nation, in the hope of improving his taste. It is true that he may join the schools and study types of ornament, but the study of ornament in the abstract, and without an adequate knowledge of the mode in which that ornament should be applied, may teach him to draw indeed, but will never teach him to design. I would rather say—make up your mind to study one branch of manufacture at one time and ascertain, as you may easily do, the best period and most notable country for that manufacture.

Choose the simplest objects first, and draw them (I assume that the student has learnt to draw) geometrically, giving an accurate profile of every vase or casket, mapping out the ornament on its surface, and making memoranda of any color decoration. In furniture and metal work examine carefully the construction of the objects, and plan it where possible, show every joint or rivet. Make full-

sized sections of the mouldings, and figure every dimension on the spot. In examining old stained glass note the disposition of the lead lines, and in woven fabrics the direction of the threads, with as much care as you bestow on the figures or patterns themselves. Studies such as these will be of more value for future reference and of far more help towards mastering the proportions of design and technical details of manufacture than any perspective sketches however elaborate.

In selecting objects of ancient manufacture for this kind of study I would strongly advise you to choose those which in some form or other are still serviceable under the conditions of modern life. No doubt in many instances they may not be so attractive or interesting as others whose purpose has become obsolete, and the very quaintness of old fashioned articles, reminding us of the habits of our forefathers, will invest them with a certain charm which it is difficult to resist. But after all, if our object be to improve the taste of modern industrial art, it seems waste of time to perpetuate types of form which are alien to the necessities of our own day. By-and-by no doubt you will be able to glean hints for decorative design from such objects, which may be usefully transferred to those of everyday use; but, speaking generally, and especially to beginners, I would recommend you first to confine your studies to that class of articles which a shopkeeper would find saleable if reproduced for the modern household.

There are hundreds of such articles now exhibited in our museums, and it has always been a matter of astonishment to me that manufacturers who at a trifling cost might provide themselves with models or drawings of these objects for imitation, prefer to bring out year after year what they call "novelties," and which, while departing widely from ordinary types of form recognized by custom, are far from realizing the grace of ancient art. Some few years ago I lent the proprietor of a well-known china shop a pretty example of old Italian majolica for table use, which I thought might be reproduced at a moderate cost. The design was picturesque and good of its kind, and the china merchant was delighted with it. Well, this piece of majolica—it was a saltcellar—was sent down to the potteries to be copied, and, after waiting some weeks, I called to see whether the model had arrived. The china merchant said he was very sorry, but there was always a little delay about these matters. It had not been put in hand, but he would write about it. He did so, and I waited for several weeks more. At last I called again, but it was not ready. More letters passed, and at the end of six months the model arrived, very correctly imitated in form, but with a plain white glaze on the ware. The pretty Arabesque pattern on its surface had not been added. I had to wait three months more for that, and when the saltcellar came from the potteries I found that the local artist had tried to improve upon the original by shading up the ornament in a modern fashion, quite different from the original. Well, I said this would not do for me. I wanted the decoration copied line for line. Back to the potteries went the saltcellar, and I had to wait three months more. At length, after exactly a year had elapsed from the time when this piece of majolica left my house, I obtained a very faithful copy of it, and congratulated myself on having helped to revive a good design. But on inquiring the price I found that each copy would cost about four or five guineas. I had given as many shillings for the original in Italy. Now whether it was due to the costliness of the reproduction, or the caprice of the manufacturer, I do not know, but certain it is that although I have frequently visited the shop since, I have never seen this reproduction offered for sale, and I might just as well have never troubled myself about the matter. Observe that this was not a question of much difficulty, nor need it have been one of much cost. The French would readily have reproduced the article in their Gien ware, and at a third of the price. Indeed, every season I see in the same shop articles requiring quite as much care in production, with surface patterns far more elaborate, but they are novelties of nineteenth-century design, and, for some mysterious reason, are preferred both by the British manufacturer and the British salesman to any copy of ancient art.

I do not say that this infatuation extends to all branches of manufacture. In cabinet work, for instance, there has been a marked improvement of late years. The coarse carving and lumpy machine-made ornament which disgraced our furniture some twenty or thirty years ago, are being gradually replaced by delicate mouldings and graceful inlay. Hand polish is taking the place of heavy varnish. English oak, American walnut, and ebonized mahogany are extensively used, and often with great success, in the design of sideboards, mantelpieces, wardrobes, chairs, and tables. Many of these articles, although designed, and with good reason, to suit the requirements of modern life, recall some of the graces of ancient woodwork. There is indeed a tendency to prettify them unnecessarily, and especially when colored decoration is introduced, in panels, etc., the design is apt to be restless and fussy in character; but on the whole these examples of furniture are picturesque, and often exhibit evidence of an educated taste which was unknown in the last generation.

In curtain stuffs and printed goods there has been a still greater advance as to quality of design, and this in regard not only to the form of pattern but to the scheme of color adopted. Dyes and subtle combinations of tint are produced which would have been pronounced impossible a quarter of a century ago, and which, if produced, the great authorities on color would probably have scorned. For there were wonderful theories extant in those days about chromatic scales and the proper proportion of primaries to tertiaries, and complementary tints; and a general impression seemed to exist among the learned that the art of decorating in color was something which you could work out by arithmetic.

I devoutly hope that those far-fetched theories have been discarded by this time, together with all similar rules which attempt to apply a scientific test to the achievements of inventive art. If a designer truly loves color, his eye will always be in search of it, and by degrees he will learn almost instinctively why such and such combinations are harmonious and pleasing, while others are crude and unsatisfactory. In that and every other branch of design, it is well to bear this rule in mind, that whenever you have to judge between two distinct kinds of effect, one depending on extravagance of form and startling contrasts, and the other on delicacy of outline and sober hues, you will generally be right to err on the side of moderation. Very clever designers, by dint of long experience, learn the art of surprising agreebly, but all surprises are not agreeable; and just as every sensible man is won more by quiet argument than by vulgar rant, so most of us will, I think, prefer temperate to restless decoration. While on the subject of color, I cannot help saying that the more I study its effect and value, whether in pictorial or decorative art, the more convinced I am that its application will be found most harmonious in instances where one dominant hue is found to which all others are subordinate. You have all heard of that famous picture—Gainsborough's *Blue Boy*—which is the delight of every painter and connoisseur. Now why has this portrait such extraordinary attraction? Chiefly, I venture to think, because it has this quality of chromatic unity. You recognize it at once as a blue picture. I don't mean that it is all indigo, or cobalt, or French blue, or Prussian blue. It may pass from one to another of these shades, and include gray, white, and green. You may get warmer hues of pink and brown in the flesh tints and background, by way of delicate contrast, but the prevailing tone is definitely *blue*.

Well, I think that our rooms should be decorated on this principle, not in the upholsterer's sense of harmony by covering all his furniture with stuff cut from the same piece and by hanging up curtains to match, but by making one color dominant and ringing a variety of changes on it. In this way yellow might lead up to green, silver-gray up to purple, and Venetian red up to brown, but the subordinate tints in each case should have a certain affinity to the dominant color, and when you have settled all this you will find that any little bit of contrast introduced, provided it be unobtrusive and does not interfere with your scheme, will have a cheerful rather than a discordant effect. And this principle concerning the decoration of a room may be safely applied, I think, to all departments of design in which the element of color

is a leading feature; as, for instance, in textile fabrics, paperhangings and the surface patterns of pottery and china. Wherever you find two or more colors introduced in such even proportions that you are puzzled to know which *rules*—so to speak—be sure the design is bad.

In the three branches of design which I have just mentioned a great change has taken place within the last few years, as no doubt many of you have noticed. I mean the substitution of natural forms of ornament chiefly borrowed from vegetable life, for the geometrical patterns and diapers which were once in vogue. This change is no doubt due to a reaction in architectural taste from mediæval types to those which prevailed during the seventeenth and eighteenth centuries. I have not time to go into this question of architectural taste now, and indeed it would be beyond the purpose and scope of my lecture, but it is quite certain that this change has taken place, and that it is gradually affecting the character of design in all branches of industrial art.

Now this will necessitate more than ever on the part of designers a study of natural forms in plants, animal life, and, indeed, of the human figure itself. In pursuing these studies I cannot remind you too emphatically that a literal imitation, or rather a literal representation, of natural forms has never in the best ages of art, and never will, constitute a good decorative design. To a certain extent, every person who can lay claim to a taste at all admits this principle. A marble bust, painted up like wax-work, a group of dogs, after Landseer, depicted on a hearthrug, or a shaded representation of a rosetree on a paperhanging, would at once be condemned as vulgar and ignorant by people who have had no artistic education at all. Well, then, the question arises how far, in the adaptation of natural forms to decorative purposes, you may go in the way of imitation consistently with good taste, and a very difficult question this is to answer in a definite way.

Perhaps the broadest and safest rule to follow in adapting natural forms to decorative purposes is to omit all expression of light and shade. I don't mean that you should be debarred from indicating, let us say the rotundity of buds or fruit, for instance, by a few dark threads in textile work, or a few "hatched" lines in the design of a wall paper, but there would be no attempt to relieve one object against another, or to give an effect of distance by the opposition of light and dark colors, and, above all, there should be no receding backgrounds or perspective in the pictorial sense of the word.

This rule is generally observed in the best curtain stuffs which are now designed, and to a great extent in the design of paperhangings also, but I have often observed it violated in the case of mural decoration by tiles and *plaques*, where I think the introduction of chiaroscuro and perspective is a great mistake. It is true that we find authority for it in some periods of Italian art, just as we find authority in the Middle Ages for painting the internal stone-work of our cathedrals, with patterns from floor to roof; but these are instances when, if ever, we may appeal to the critical philosophy of modern taste for our guidance, and most modern critics, who have given attention to the subject, agree on this point.

Among other branches of industrial art there are three which deserve especial mention, viz., stained glass, ornamental metal work, and the manufacture of pottery and porcelain—each of which has within the present generation been marked by extraordinary improvement either in design or manufacture, though not to an equal extent in both. I have purposely refrained from mentioning names in this Paper, but there are certain firms with whose work as glass-stainers you are all familiar, and who deserve the greatest credit for the high artistic excellence of their inventions. Personally, I think that academic accuracy in the delineation of the human figure is of small importance in a stained glass window, and for this reason: The very conditions of the art are not only removed from, but opposed to, pictorial treatment. In pictures, as Mr. Ruskin once deftly pointed out, the lights are opaque and the shadows transparent. In stained glass it is exactly the reverse. The highest light is the most transparent portion of the glass, and the darkest shadow is represented by that which is most obscure. In short, a stained glass window is less of a picture than a piece of tapes-

try. This being so, and inasmuch as in stained glass a judicious arrangement of color is the one essential for excellence, it seems unreasonable to hamper the design by the introduction of forms which prescribe any limit to that arrangement. One of the secrets of excellence in early glass was that the old designers made form entirely subservient to color. The quaint angular outlines of ill-drawn figures lent themselves more readily to a free association of cunningly opposed tints than if the artist had been restricted by considerations of anatomy and perspective.

Apart, however, from this question, and accepting the fact that modern designers do aim at correctness of form in stained glass, I think we must all admit that many works of the present day do realise great beauty of color, and I for one entirely approve the spare use of positive tints, which may be reckoned among its characteristics; for nothing can be more destructive of effect in internal decoration than a flood of crudely variegated hues pouring in from a window.

In the design of ornamental metalwork there seems to be no lack of inventive power, and though, being chiefly used for ecclesiastical purposes, it generally assumes a mediæval form, efforts have been recently made to produce articles of domestic use which shall partake of a character more in unison with the latest phase of architectural taste. In point of execution it would be the better for a little more refinement in hammered work. Sheet metal might be used in many cases where castings are now employed, and if this should result in substituting a happy freedom of outline for the mechanical accuracy which still seems to be the ignoble aim of the modern workman, it will be all the better for the interests of art.

The revival of majolica ware is an incident in the history of British manufacture of which the present generation may well be proud. In quality of material and skill of manipulation it leaves little to be desired. It is, however, far more expensive than it need be, and this is because the artists employed in its decoration insist in finishing their work too highly. Old majolica, intended for domestic purposes, was not treated in this way, but sketched upon in a free and easy fashion, and though we find elaborately painted plates and dishes in our museums, they may be regarded as exceptions to the general rule—articles to be hung on a wall, as we hang water-color drawings now, and not destined for ordinary use.

The French in reviving this manufacture have had both objects in view; and while their more costly productions are of the kind I have mentioned, they also supply to the public a cheaper class of goods printed in imitation of Italian faïence and old Rouen ware which, in point of design and price, we have not yet rivalled. English porcelain is perhaps the best in the world so far as quality of material is concerned, but the dinner and tea services which we see in our shop windows are still far from satisfactory in design, and the thick lustrous glaze with which they are covered is, to my mind, quite inconsistent with artistic excellence of effect.

I should like to have said something more on this subject as well as on the design of dinner plate and other fields for the exercise of "industrial art;" but I fear I have already exceeded the limits of the time allotted to me, and perhaps too the limits of your patience.

In conclusion I will only record my conviction that a time has arrived when the public of this country are becoming sincerely interested in the progress of "household taste," and that it will rest with designers educated in this and other schools of the same kind to determine by their efforts the direction which that taste should take and the success by which its development may be attended.

A Shovel Manufactory.—The Ames Shovel Manufactory at North Easton, Massachusetts, gives employment to 500 persons, who turn out 500 dozen of shovels, spades and scoops daily, which will find their way all over the country as well as into foreign lands. The company's shops in North Easton are built of stone, and if placed in a line would extend for half a mile. In addition to the work done at North Easton, shovels are made at Canton, South Braintree and West Bridgewater, and are brought to North Easton for the finishing touch.

THE ST. GOTHARD TUNNEL.

By Prof. COLLADON.

Translated from "Revue Universelle des Mines."

The St. Gothard Tunnel has no comparable precedent, excepting that of Mount Cenis, finished in 1871, and the Hoosac Tunnel, completed in 1874.

The gallery of Mt. Cenis, 12,233 meters long, was undertaken by eminent engineers at the expense of the Government, and for which no necessary pains have been spared. It has taken *thirteen and one half years* for its completion.

The total length of the Hoosac tunnel is 7,634 meters, and the mean progress was less than that realized at Mt. Cenis.

The St. Gothard tunnel, constructed in harder rock, and 14,920 meters long, is a Swiss enterprise, and, according to contract, must be completed in eight years, or nine at the most.

On account of the excess of length and the shortness of the time allowed, this tunneling must be accomplished six times as fast as that of Mt. Cenis.

Can this gallery of St. Gothard be completed in eight or nine years is the main question which justly occupies the mind of the industrial world.

This question of *time* depends upon the mode of execution, and involves some technical principles upon which engineers are far from agreeing.

A tunnel having a double roadway, as that of Frejus or Gothard, requires an excavation of eight meters in breadth and six meters in height clear of the masonry.

We will not immediately consider this large section, but merely of a small gallery, having about $2^{m},40$ in height and $2^{m},60$ in breadth, and kept about 200 or 250 meters in advance.

This small gallery is bored by means of a machine worked by compressed air, which produces at the same time power and ventilation, according to the process proposed by M. Colladon, in 1852 for Mt. Cenis. The boring apparatus, called perforators, invented by Bartlett in 1855, and modified by the celebrated Sommeiller in 1857, have become much multiplied and improved; we can count, to-day, twenty or twenty-five different modifications of this apparatus.

The two methods, that of boring the small gallery at the top, and that of boring it at the bottom of the great section have their more or less exclusive partisans.

The Mt. Cenis tunnel was commenced at the bottom; the Hoosac tunnel adopted both systems; M. Favre prefers piercing St. Gothard at the top, furthermore he employs the machine drill both for the advance gallery and for the lower stages. The rapidity with which the work progresses indicates that it is a good method. The first gallery (*galerie d'avancement*) is bored to the height and breadth of about two and a-half meters, having a section of six or seven square meters. As the entire tunnel will be arched, it is necessary to excavate more space for the masonry in order that the roof of the gallery may be $6^{m},50$ or 7 meters above the bed of the proposed railway.

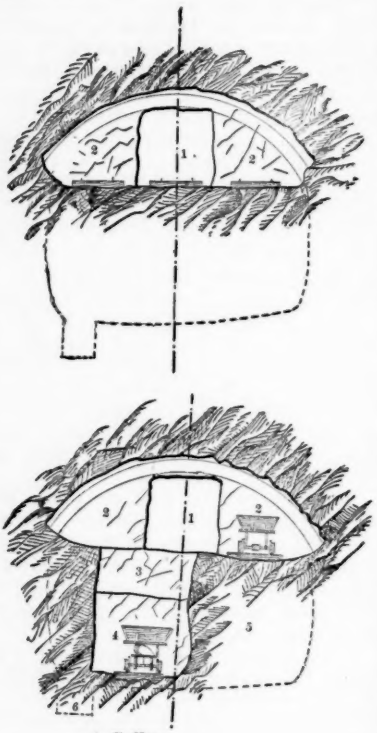
We know that tunneling by the use of powder or dynamite is very dangerous for the engineers and workmen. The excavations are therefore made in separate places and at distances necessary for the safety of the men at the apparatus.

The end of the gallery is called the working place or heading. To the right and left of the gallery, and from 200 to 250 meters in the rear of the heading, are excavated two segments or side areas to the full width of the arch. These are called *abattages*.

200 or 300 meters behind the *abattages* is opened a ditch (*cunette du strosse*) nearly to the floor of the tunnel, that is to say four or five meters below the floor of the gallery; it is about three meters wide. In the rear of the heading of this ditch are excavated the lateral portions (*strosse*), which, when excavated, opens the entire section and the masonry is begun.

The *abattage* and the gallery have their own special little railway, another is built on the floor of the *cunette*.

Numerous cars are continually running on these roads, bringing in tools, provi-



1 Gallery.
2.2 Abattages.
3.4 Cunette de strosse.
5 Strosse.
6 Drain.
Scale, $\frac{1}{210}$.

sions and materials, and carrying out debris.

It is clear that the progress of the entire work depends on the progress of the gallery. At this heading the rock encased on all sides resists the effort of the explosion, and but few men or machines can be employed here, whilst in the enlarged portion of the tunnel the number of both may be very much increased.

The progress realized at the heading by M. Favre and his able engineers is very wonderful, especially if we compare it with that which has been done in rock of similar kind.

It is right to believe that this advancement progressed in the small gallery of Mt. Cenis to the end. For the years 1868, 1869, and 1870 the heading was advanced, 1320, 1431, 1635 meters, affording a maximum of 409 meters per quarter.

At Mount Hoosac, notwithstanding the employment of nitroglycerine and more rapidly acting perforators than those at Mt. Cenis, the total quarterly advancement for the latter years have been: 207 meters in 1870, 238 meters in 1871, 237 meters in 1873.

Here are the figures of the progress in the last five quarters at St. Gothard.

	Göschenen.	Airolo.	Total.
From July 1 to Oct. 1, 1874,	321m,70	174m,10	495m,70
From Oct. 1, 1874, to Jan. 1, 1875,	283m,60	243m,30	526m,90
From Jan. 1 to April 1, 1875,	267m,90	289m,10	557m,00
From April 1 to July 1, 1875,	312m,10	344m20	656m,30
From July 1 to Oct. 1, 1875,	360m,90	326m,20	687m,10

The prophets had announced that M. Favre, hindered by the excesssive hardness of the rock and a greater quantity of water than was found at Frejus or at Mount Hoosac, would not be able to exceed an average of three meters per day at each heading, or, at the most 550 meters per quarter. In the last three quarters this maximum is changed to the minimum. The last two have given 3m,60 and 3m,80 of average advancement daily for each heading.

During this period there has been some time lost for examination, by direction of the Company, and by a temporary strike at Göschenen.

Notwithstanding these excellent results the ill-will is not destroyed. They predict anew that the tunnel will not be finished in eight years—the total bulk extracted being insufficient, etc., etc.

We will remark, at first, that not *eight* years, but *nine* years are allowed by the agreement, *with enormous penalty, it is true;* but this limit is only the more honorable in sight of rational men; and certainly when a contractor gains for a company fifteen millions he has a right to all the regard and to the extreme good will of those who represent that company, and of all men of this trade, especially in view of a success which is already a great honor to the Swiss.

It is evident that progress in works without precedent, and where diffi-

culties seem to accumulate, ought to be purchased by trials, study and successive improvement. The whole progress cannot be accomplished at once, nor in a single year. The work at Mt. Cenis, where the expense was less and where each kilometer paid double that of Gothard, it was shown that the engineers and contractors of Frejus continued to improve in their machines and in their organizations of labor. M. Favre considers it to be his duty first of all to make progress at the heading. In three years he succeeded beyond all expectation—thanks to his activity, energy, ingenious spirit and enormous sacrifices. Now they contend that the enlargement should progress equally fast. It is hoped that, when the improvements and projects in progress shall have had time to be completed, experience will prove that the fault found with the able contractor has been premature and unjust.

The elements of progress in drilling very long tunnels depend, essentially, upon the use of machines and abundance of motive power.

The force is transmitted by compressed air which operates the perforators and at the same time ventilates the tunnel throughout. To obtain this power, waterfalls, motors, and compression apparatus are necessary. We will speak first of sources of motive power, afterwards of air-compressers, and finally of engines which this air puts in motion.

II. WATERFALLS AND MOTIVE POWER.

On the south side of the tunnel can be utilized the Tessin or the Tremola; in the last stream, which descends from Lake Sella, above the hospital, the water flows smoothly over a declivity of 20 to 100, an advantage which should make it desirable. From previous gaugings it is calculated that its minimum volume of water would be three or four hundred litres per second. To make the best possible use of this small volume it was necessary to obtain a maximum of fall, consequently the height of the upper reservoir above the turbines has been carried to 180 meters or 18 atmospheres. This source presented, apparently, insurmountable difficulties; this stream, flows through a gorge where large avalanches fill its bed nearly every minute.

M. Favre conceived an ingenious idea of bringing the water from a higher point, where the Tremola is accessible all the year by means of a canal of 1000 meters;—he directs the water into the bed of a secondary stream, the Chiasso, more remote from the fall of avalanches.

The depositing reservoir which ought to retain the floating matter and gravel is at the side of the Chiasso under shelter of a rock.

Clear water from this reservoir, placed 180 meters higher than the hydraulic motors, descends by a conduit $0^{m},62$ in diameter, and 841 meters long, formed of very strong iron tubes, to the motors and compressers near the workshop.

The four turbines were furnished by Escher, Wyss & Co. of Zurich.

These turbines have vertical axles—they are $1^{m},20$ in diameter, 100 floats, and make about 350 revolutions per minute.

They are cast in a single piece with their floats in bronze; under excessive pressure bronze lasts longer than iron, cast iron or steel. The shock of water under eighteen atmospheres affects the last three metals at the end of a few months' service.

Each of these turbines drives by a single gear a horizontal shaft, and all these shafts are placed on the same line.

The performance of this apparatus has been most satisfactory; it fails only in times of excessive cold or great droughts. This volume has decreased, during short intervals, to less than 100 litres per second. M. Favre decided in 1874 to obtain from the Tessin additional power.

This stream at its head at Airolo seems to defy all attempts to utilise it; its declivity is small, and it flows between rugged banks composed entirely of crumbling rocks, and to increase the danger, avalanches of snow and rocks fall every winter.

He, however, undertook this perilous work and has succeeded. The supply canal discharges a cubic meter per second: it is in a great measure suspended to the sides of almost perpendicular rocks, through two aqueducts raised twenty-five or thirty meters above the Albinasca and Tremola rivers.

The project of raising the water of the Tessin to the reservoir of the Tremola by means of a canal seven kilometers long was impossible, consequently, he

determined to make a canal of three kilometers and a second reservoir, placed only ninety meters above the turbine wheels.

There are, then, at Airolo two supply canals, and two depositing reservoirs situated at two heights, of which one is double that of the other.

The corresponding difference of velocity for the two falls, being in the ratio of two to three, it was necessary to have recourse to two kinds of turbines. These important additions were accomplished in a remarkably rational and happy manner, by placing, under each shaft of the turbine wheels, a second turbine of different dimensions calculated for the fall of the Tessin.

These four new turbines are of the Girard system; each has its supply of water and special flood-gate. They were constructed and put in place by Escher, Wyss & Co.

This addition made the whole very easy to regulate, and assured a regular action throughout the year.

The water of the Tremola being freer from *debris* and gravel is always preferable, but as it has not the required volume, the water of the Tessin acts as complementary motive power; or if the conduit of eighteen atmospheres had a break or was interrupted in any way, the conduit of the Tessin would be able to supply its place.

The second work of canalization, so eminently remarkable for its energetic and judicious execution, has fully attained its aim. Its essential results are to regulate and at the same time increase the moving force. It now affords to the workshops of Airolo a power equal or superior to a thousand horse.

On the side of Goschenen the valley is also exposed to earth and snow avalanches, but here they are less frequent and less powerful, are of short duration, and present no other serious difficulty than that of transforming for a day or two the snow, which obstructs the wire lattice and the conduits, into mud, and occasions a delay that nothing can prevent.

The Reuss, below Andermatt, rarely decreases to less than a cubic meter per second; its slope is about 10 to 100, and permits of the construction of a useful fall of 85 meters by placing a dam about 926 meters up the river from the mouth of the tunnel.

130 meters downwards from the dam is a depositing reservoir of 100 cubic meters divided into five compartments and three chambers, serving to discharge the sand and gravel and retain the floating matter.

The last chamber affords exit to water by sheet-iron pipes being $0^m,85$ in diameter, and 801 meters long, which descend to the building of the four turbines and distributes to them a total volume of about twelve hundred liters per second.

These four turbines are of the Girard system and have horizontal axles; their diameter is $2^m,40$; their normal velocity, 160 revolutions.

III. AIR COMPRESSERS.

The quarterly report, No. 5., of the Federal Council, published in 1873, contains details of the air compressers used at Mt. Cenis, and compares their execution with those of the air-pumps adopted for Gothard. It revealed that, in the year 1852 M. Colladon had proposed, in a detailed memoir remitted to the Government, the use of compressed air as a substitute for cables in transmitting force into the tunnel. This memoir contained :

1st. The results of numerous experiments that had been made in 1850, 1851 and 1852, upon the resistance of air and gas in tubes of different diameters, and upon the essential modifications resulting from these experiments, upon the coefficients of resistance depending on the diameters.

2d. The calculations for application at the Mt. Cenis tunnel.

3d. Some practical details of compressed-air pumps, of the transmission of force, of its storing, of compressed air used for throwing water, and of the means of conducting the power at the end of the tunnel for setting in motion the drilling machines.

In this memoir, M. Colladon proposed to utilize the waterfalls by means of turbines; he indicated the possibility of cooling the compression pump by an envelope of water, or by an interior injection, and speaks also of liquid piston pumps.

These projects were presented, in 1852, for the boring of Mt. Cenis, but it was

found that they were only partially applied, principally in the employment of compressed air as a substitute for cables; they are found now to accomplish wholly the work at St. Gothard.

MM. Sommeiller, Grandis and Grattoni, entertaining the idea that trains could be made to run on inclined planes by air motors, had, in 1853, taken out a patent for an air compressing ram. The experiments had been abandoned, when, in 1857, the Government decided to undertake the tunneling of Mt. Cenis and trusted the execution to MM. Sommeiller, Grandis and Grattoni, whose ram appeared, at that time, most advantageous for the compression of air.

Consequently, twenty compresser rams were ordered to be distributed near the two mouths of the tunnel. Their total cost exceeded two million francs. The practical results were not favorable, as the machines remained without employment at Modena and were not used for the space of three years at Bardonneche. For them were substituted double cylinder pumps containing two and a half cubic meters of water which formed liquid pistons for the compression of air. These pumps were, at that time, considered a great improvement.

An official report of three engineers, published in 1863, states that they gave, with the same hydraulic force, three times as much air as the rams, and at a third less cost.

It is easy to see that pumps with an alternating motion, whose piston is to move a great quantity of liquid, are not susceptible of rapid oscillations. This has been confirmed by experiments with all compression apparatus constructed on this plan. Beyond a certain limit of velocity any increase of power is not attended by useful results.

At Bardonneche, as well as at Modena, they had to limit the number of revolution of the shafts to eight. To compensate for this the pumps employed must have excessive dimensions.

At Gothard, as in all mountainous countries, the best hydraulic motors are rapidly revolving turbines connected with high falls.

For the compression of air at Goschenen and at Airolo, it has been necessary to apply similar engines to those used at Mt. Cenis; between the turbines and the pumps they had to interpose numerous powerful brake wheels to properly reduce the velocity, to prevent loss of work, chances of accident, frequent changing of apparatus and above all great excess of expense.

The employment of turbines necessitated that of rapidly moving compression pumps; but it was necessary, at the same time, to prevent heating the air which would have involved a very notable loss of useful effect.

Prof. Colladon had taken out a patent in 1871, for a new system of air-compression pumps, which, by a very rapid action, permitted of compression of the air or gas, and at the same time annulled the prejudicial effects of heating. A pump of this kind was established in 1871 for the railroad in Upper Italy; this pump, designed for the compression of illuminating gas under high pressures for lighting trains at night, worked constantly nearly a year at the mean velocity of about 200 strokes per minute.

This result was obtained by a double combination, which, simultaneously, cooled the outside and the moving parts of the pump; the cooling was accomplished by the injection of a very small quantity of spray. The piston and its rod, prolonged beyond the cylinder, are hollow; their interior is constantly cooled by a small stream of fresh water brought by a tube to the hollow part of the rod. This water circulates in the cavity of the piston, and goes out through the space between the injection tube and the sides of the rod.

For pumps of great volume the cooling is completed by small injectors which mixes the air with the spray.

Pumps of this kind were put on trial in the workshops of the Geneva Society in presence of the contractor M. Favre; he made known the possibility of obtaining great volumes of air, under pressures of eight or nine atmospheres without injurious heating.

The turbines of Airolo, each of 200 horse power, made 350 revolutions per minute. The consulting engineer proposed to employ pumps making eighty revolutions in the same time and worked by tangential wheels with interposition of only one gear. To equalize the resistance and dispense with the use of fly-wheels, the engineer coupled these pumps

in groups of three, placed parallel on the same base, and worked by a shaft with three cranks. This plan was adopted by the Company. MM. Escher, Wyss & Co. had charge of transmission, and the Geneva Society of Construction had the supply of five groups of these compressers each, for the Airolo side.

These five groups are placed with moving turbines in a chamber 35 meters long and $8^{m},50$ wide. Each turbine can work either one of the groups, or all of them together.

Four of these groups working together can supply, hourly, to the tunnel nearly a thousand cubic meters of air at the tension of seven or eight atmospheres, which, before expanding in the tunnel can transmit to the working machinery a force of several hundred horse power. This volume, in expanding, furnishes for ventilating the tunnel eight thousand cubic meters of air.

The disposition adopted for the air compressers at Goschenen differs from that of Airolo only in some secondary details. The pumps are disposed in like manner; they also form five groups, each composed of three compressers. The crank shafts have a mean velocity of sixty revolutions per minute. This difference of velocity compared with that of the apparatus at Airolo, is compensated by an enlarged volume of the pumps.

The compressers of Goschenen were furnished by MM. Roy & Co; they are constructed after the Colladon system and differ only in some details from the compressers that the Geneva company have provided for Airolo. In each of the stations the compressed air is collected into sheet iron cylinders serving as reservoirs. Thence it is carried by a continuous tube of $0^{m},20$ diameter to the extremity of the cunette. This air is immediately conducted to the *abattages*, and to the extremity of the gallery by means of heavy iron tubes $0^{m}14$, and $0^{m}10$ in diameter. The outlets of air for working the perforators were established by means of rubber tubes $0^{m}05$ in diameter.

Besides these supply cocks, there are at several points of the principal conduit air cocks for ventilating purposes.

The Colladon pumps working for the last two years at Gothard show, in an unquestionable manner, the possibility of compressing large volumes of air without hydraulic pistons to the tension of eight atmospheres, or more, by rapidly moving pumps, and of obtaining this compressed air at an elevation of temperature not exceeding 12° to 15° C.

On the Bardonneche side of Mt. Cenis, for supplying compressed air, they established seven bucket wheels, each of which was coupled to four large water-column cylinders. To put up these wheels and their pumps, they had to construct seven distinct buildings, each having a surface of 300 sq. meters.

These seven wheels and their twenty-eight compresser cylinders can furnish, per hour, 570 cubic meters of air under pressure of six effective atmospheres. This air by its expansion gives for ventilation, about four thousand cubic meters at atmospheric pressure.

At Gothard, four turbines driving twelve pumps of small capacity with great velocity, produce 1,000 cubic meters per hour at a tension of seven effective atmospheres; and this air in its expansion in the depths of the tunnel furnishes 8,000 cubic meters at the pressure of the atmosphere.

These turbines and their compressers are, to a great extent, in a single building which has a surface of only 350 square meters.

After all, the Colladon pumps of great velocity driven by four turbines give at Gothard twice as much power in compressed air as the apparatus used at Mt. Cenis, at about one third expense and occupying one fifth the space.

Eighty of the same formerly employed at Mt. Cenis are equivalent to four turbines and twelve pumps like those at Gothard.

IV. VENTILATION OF THE TUNNEL.

The average number of men working on one side of the tunnel at the same time, is four hundred.

Each one is generally provided with a lamp, and each lamp requires a renewal of air equal to that necessary for the man. An average of thirteen cubic meters of fresh air per hour is required for one workman and his lamp, or five thousand two hundred cubic meters per hour for 400 workmen and their lights.

The mean quantity of dynamite con-

sumed in twenty four hours, at each mouth of the tunnel, is estimated at 300 kilogrammes, an average of twelve and a half kilogrammes per hour. It is proper for good ventilation to give one hundred cubic meters of air with each explosion of a kilogramme of dynamite, corresponding to an average of 1,250 cubic meters per hour.

It is required, then, to introduce at each side of the tunnel, six thousand four hundred and fifty cubic meters of air at one atmosphere, per hour. We have seen that, at Airolo as at Goschenen, the turbines drive four groups, and send into the tunnel the equivalent of eight thousand cubic meters under the atmospheric pressure.

This volume would be more than sufficient, if the fresh air expelled the same volume of partially vitiated air. This effect is produced in a satisfactory manner in the gallery and at the *abattages;* but as the excavations increase an eddy is produced and the foul air remains behind in the cavities or under the arch, while the fresh air partly escapes outward.

In order to remedy this imperfect expulsion, the contractor has decided to place at each end of the tunnel a powerful system of suction, extended to the extremities of the arch by a continuous tube $1^{m},20$ in diameter suspended under the intrados.

This apparatus is composed of two connecting bells suspended at the extremities of a beam, and which receive alternate ascending and descending motions by the working of two water-column machines. Each bell is plunged into an annular tub full of water; the central part of this tub is closed by a diaphragm supplied with valves, and the bottom of each bell is also provided with clack valves opening outward. At each ascension the air is drawn through the entire length of the tube suspended from the arch, and when the bell descends the air is discharged into the atmosphere.

The two bells are able, by ten double oscillations of the beam, to draw 500 cubic meters per minute, or 30,000 per hour.

This air, drawn to the amount of many hundred meters from the bottom of the tunnel, should be replaced; it will be, in part, by the 8000 meters furnished by the pumps, while 22,000 cubic meters per hour ought to arrive from the outside through the large section of the tunnel already finished.

With this powerful ventilation, which will be increased by the new pumps, the interior of the Gothard tunnel will, certainly, be better aired than is the majority of mining works.

V. BORING MACHINES.

The work at Gothard has given rise to new boring machines and to important improvements in the construction of this useful apparatus.

The first boring machine intended for piercing hard rock, by employing compressed air, was constructed in 1855, by the English engineer, Th. Bartlett, representative of M. Brassey, builder of the Victor Emmanuel Railroad. This remarkable machine was tested in March 1857, at Coscia, in presence of a nominated commission in view of the Mt. Cenis tunnel.

M. Sommeiller assisted at these experiments, and the rapid action of this machine induced him to have it patented, and it was employed, to the exclusion of all others, at piercing the tunnel of the Cottiennes Alps.

Then at the International treaty for the Gothard railroad, the Italian Government made, as a condition of its support, the re-purchase, by the Swiss Government, or by the executive Company, of all the old material which had been used in piercing at Frejus. This re-purchase was one of the obligations imposed upon the builder when he signed his contract. M. Favre, it is seen, bought one hundred of the Sommeiller boring machines for his own use.

This method is no longer in use; improvements and new ideas have arisen and have effected many changes; there are, to-day, more than twenty different machines for boring blast holes in hard rocks.

All of these machines have essential analogous parts and are generally composed:

1st. Of a principal cylinder for percussion.

2d. A percussion piston from which the shaft is prolonged and serves as a head-stock, because they fix, at its extremity, the chisel, the graver or the

foil used for piercing the holes in the rocks.

3d. A slide-valve, or governator-cock, whose alternate motion guides the compressed air, alternately, before or behind the piston.

4th. Gearing for turning the piston, its shaft, head stock and piercing chisel, and to make the cylinders and these attachments advance against the working place during the progress of the drill.

5th. A support or frame, usually parallel tracks, on which the cylinder and these attachments are able to slide as the work advances.

This frame or support, intended to be placed on a carriage, should be able to incline in different ways according to the direction of the holes which are to be drilled.

The drill ought to have a rapid and powerful alternate motion; it ought, also, to turn on its axis to disengage its edge, during the perforations, and make a straight and regular hole. The piston and the shaft head-stock ought, evidently, to participate in the same motions. Finally, the cylinder and the principal attachments ought to advance, whether by hand or automatically, toward the work.

The miner, with a hand-drill realizes in an admirable manner these three simple indispensable motions; but the muscular force of a man is insufficient when the drilling is to be rapid. It is necessary then to have recourse to the use of machines, and to those of compressed air, especially when we wish to work in the depths of a tunnel.

Side by side with the accomplishment of the three above named motions there are other elements of comparison which guide the contractor in his choice of boring machines, such as: the proportion of compressed air for certain effects produced; the good execution of the apparatus; the choice of metals employed in its construction; the first cost; the cost of keeping it in repair; the more or less easy management by the workmen; the weight of the machine; the length and breadth and the depth of hole that it is able to make in one operation without changing the drill.

The Company of Gothard made trial of several models of boring machines, both at Geneva and at the tunnel. After these trials they were limited to the use of three or four models, each of which had its special advantages. The variety of these systems was no detriment to the rapidity of execution of the work, for the company required of the constructors, that each perforator should be adapted at once to the principal carriage on which they were to place a number to work together; they required also that the mode of operation be so easy and simple that miners be able to work after a very short apprenticeship.

The experiments show that this mode of manufacture is preferable to that used at Mt. Cenis.

Immediately after signing the contract, the contractor decided to purchase, in Belgium, two steam compressers and a provisional water-column, which were placed at the northern and southern extremities of the tunnel. He treated, at the same time, with the manufacturers, Dubois and Francois, for the delivery of a limited number of their boring machines.

These have points of resemblance with those of Mt. Cenis, but are different in several essential particulars.

Sommeiller's machine is composed, in imitation of that of Bartlett, of two distinct parts; one very small compressed-air motor with a fly-wheel, and a perforator properly so-called. It was by means of this small motor that Sommeiller was able to move the distributing valve and obtain the rotation of the piercing piston and the progression of the cylinder.

MM. Dubois and Francois' apparatus is more simple than Sommeiller's and uses less compressed air for the same effect. These manufacturers have supplanted the little auxiliary motor. The slide valve receives its alternate motion by the action of the compressed air upon two, small, unequal pistons, and by the cross-head which at each return opens a plug and determines the advancement of the valve.

The rotation of the piston and of the perforating tool is obtained by the alternate action upon a bent lever of two small pistons placed at the sides of the cylinder, and rising in turn by the impulsion of compressed air which rests upon the two faces of the large perforating piston.

The oscillatory motion of the lever causes the rotation of the head-stock, by means of a ratchet-wheel which governs the head-stock and of a catch whose motion is similar to that of the lever.

The regular advancement of the system, according as the aperture increases, is governed by a large screw parallel to the cylinder which is moved by hand. Soon after putting these machines in use, another new system obtained great success in England. After some trials made in Switzerland, M. Favre decided to order a certain number of these machines of the American inventor, Mr. MacKean.

This Anglo-American system of boring machines was entirely different from those we have tried to describe. The revolution of the piston, of its shaft and of the piercing instrument are obtained by the oscillation of the piston, by means of two screw-shaped cog-wheels.

One of these wheels is fixed on the piston rod, the other is fixed on a small, special shaft. This second shaft carries besides a ratchet-wheel.

One wheel participates in the oscillations of the piston; the pressure of this wheel against that of the second wheel tends to give to the latter and to its shaft an oscillatory rotary motion. But the ratchet-wheel and its catch will not permit the revolution of this second wheel, excepting in one direction. The result at each return of the piston is a partial revolution of the perforating piston and of the chisel.

The slide valve of the McKean machine is cylindrical, and the mechanism which moves it is more simple than in the Sommeiller and Dubois apparatus.

Mr. McKean has, for the advance of the cylinder and these accessories, employed a screw parallel to the motor cylinder. He has, also, utilized the alternate, rotary motion of the shaft of the slide valve for obtaining an automatic advance by means of a screw to which is adapted a ratchet-wheel.

The velocity of perforation obtained by this machine notably surpasses the machines previously described.

In the experiments made in Switzerland they obtained, with a pressure of from four to five atmospheres, a normal advance of $0^m,10$ to $0^m,12$ per minute in a very hard block of granite.

The whole apparatus is shorter and occupies less space than the machines of Sommeiller, or of Dubois and Francois. Its weight is also less which renders it easier of transportation and of putting into place. These machines fixed upon special small carriages have rendered useful service in the work of enlarging the tunnel.

The first McKean apparatus was illy adapted to the large carriages used at Gothard in the gallery of direction, at the *abattage* and at the cunette. In the year 1875 the inventor surmounted these difficulties, and the contractor of the tunnel decided to make a new order of sixty of these machines to work on the Airolo side.

A third system gives, also, good results at Gothard; it was invented in 1874 by M. Ferrono, the former head of the workshop at Modena.

M. Ferrono employs a separate little machine; abandoning Sommeiller's complicated mechanism which works the spider valve, he has replaced it by an eccentric to which the little motor transmits a direct rotary motion. The mechanism for the rotation of the drill on its axis are nearly the same as in the Sommeiller apparatus.

These machines employed for eighteen months at the heading at Goschenen are preferable to those of Dubois and Francois on account of the ease in working and the rapidity of advancement; they have, however, two objections: the length and weight increases by the addition of a second cylinder, and the outlay of air is considerably more than in the other machines, on account of employing a secondary motor.

M. Turrettini, the intelligent director of the workshops of the Geneva Society of Construction, has invented an entirely new boring machine.

This patent had its piston composed of two parts which separated a little before the shock of the chisel and gave more elasticity to the blow. This same shock of the drill determines the change of the distribution and the return of the head-stock; they thus avoid the serious difficulty which often presents itself in the generality of boring machines.

The revolution of the piston, also the motion of the valve, are obtained by combinations of which experience has

shown the efficacy as well as the moderate expense of keeping it in repair.

Finally, the progressive, automatic advance of the perforating cylinder along the main support, and if necessary, its recoil, are obtained by an entirely new process. It is by the principle of utilizing the reaction of compressed air, that the engineer can obtain either of these effects by a single motion of the tap. A lever acted upon by this compressed air gives to the apparatus, in every position, the needed stability for resisting the shock.

The works for automatic advance of the perforating cylinder upon the main support, have been the main difficulty in the greater part of the apparatus invented since the boring of Mount Cenis.

Some give an advance disproportioned to the progress of the drill; others employ the delicate parts liable to injury.

The automatic motion, invented by M. Turrettini, leaves but little to desire; it exactly suits the progress of the chisel, and works without shock, and is remarkably simple.

If the apparatus, supplied with its graver, is moved upon these main supports to any distance from the face of the works, at the moment when the compressed air-cock is opened, the perforating cylinder rapidly advances until the chisel reaches the rock, and from that moment continues to advance equally with the drill.

This small machine is shorter and weighs less than those of Dubois and Francois, or Ferrono.

It also consumes less air for the same work. It is, doubtless, destined to win success in the future, since it was able on first trial to contend with the best models.

These new boring machines, put in motion at Gothard, in the summer of 1875, worked concurrently with the three systems previously described, and the good results of these trials decided the contractor to order, of the Geneva Society of Construction, thirty-two other boring machines of the Turrettini system to be delivered without delay. The holes have generally a depth of $1^{m},10$. The number of those that they have drilled at the face of the works in the gallery of advancement, where the surface equals six or six and a-half square meters, varies with the nature and hardness of the stone; it is generally comprised between sixteen and twenty-six.

When the holes are drilled the carriages are moved to a distance of sixty or eighty meters, the holes are charged with dynamite, and two or three successive explosions are made. The rubbish is removed by hand or in baskets and put into small wagons and conveyed beyond the *cunette*. Then, by means of a chute they empty the small wagons into larger ones, which are stationed at the bottom of the *cunette;* a compressed air locomotive then takes ten or twelve wagons full to a place for rubbish outside of the tunnel.

VI. COMPRESSED AIR LOCOMOTIVES.

Two compressed air locomotives at each end of the tunnel are occupied in transporting. The old one is formed of an ordinary locomotive of twelve horse power, supplied with compressed air by a cylindrical reservoir of sixteen cubic meters in volume, borne on two trucks drawn by the locomotive; this reservoir is supplied by an air-holder on the conduit principle of compressed air. The other locomotive was made at the Creusot; it has no tender and is composed of a reservoir of seven cubic meters able to resist 14 atmospheres. To this reservoir are fixed two motor cylinders which work at a mean pressure of five atmospheres. The distribution of compressed air is regulated by an automatic apparatus invented by M. Ribourt, engineer employed at Gothard, and formerly pupil at the *Ecole Centrale*. This work perfectly fulfills its purpose.

To obtain a regular supply of compressed air at 14 atmospheres, M. Favre instituted special reservoirs, and in 1875 ordered, of the Geneva Society of Construction, eight compressors of the Colladon system, each being able, without sensible warming, to compress twelve cubic meters of atmospheric air per minute at the pressure of 14 atmospheres. Four of these machines work at Airolo and four at Göschenen. They are placed in the chamber where they are found uniting the four hydraulic motors and the five groups of compressers, and they are worked by the motor shafts of these four turbines.

CONCLUSION.

Before closing this abridged description of the numerous and powerful machines employed at Gothard, and of the work in progress, I should commend the intelligence, activity and energy of the eminent contractor as well as the zeal of his assistants.

When they reported at the end of 1872, that everything was against them, that there were difficulties that it was impossible to overcome or prevent, that duties accumulated and there was a combination of things to accomplish, we cannot but admire the present condition realized in less than three years.

The local and physical difficulties at Gothard have been exceptionally serious; utilizing the torrents, for example. The nearly perpendicular cliffs of rock, the exposure to falling stones and frequent avalanches render canalization on the south side nearly impossible.

To the difficulties of climate, locality, and the great mass of snow, are added, in the Airolo tunnel, incidents of greater importance: as those occasioned by the variable nature of the rock, the numerous breaks from which the sand and gravel flood the gallery, and especially by infiltration, of which the volume and the violence are extraordinary.

The streams escaping from the top and side of the south part of the tunnel, which has a descent of only one in a thousand, have transformed the lower part of the gallery and the *cunette* into a river at the bottom of which it is necessary to maintain a road and to work at the lower borings.

Two or three examples will show the magnitude of this obstacle.

At Mount Cenis the maximum of infiltrations at either mouth has not surpassed *one* liter per second. At Mount Hoosac, according to official reports, they considered eighteen liters per second a very great obstacle which notably interfered with the work.

In the first public report from the *Direction di l'Administration*, in speaking of infiltration in the south of the tunnel, which at that time had reached from fifteen to thirty liters per second, the honorable reporter called this flow: "A little torrent and a delivery of water of extraordinary proportions."

This little torrent became, some months later, a river gauging from two hundred to two hundred and thirty liters per second, eight hundred thousand liters per hour, in a gallery of less than seven square meters.

These physical obstacles are not all that have troubled the contractor and delayed the work. There have been others less known to the public, and quite foreign to the duties and the occupation of M. Favre.

He would have been ready to commence drilling the last of August, 1872.

The chief engineer of the company had illy prepared for the difficulties at the approaches. On the side of Airolo they were not ready until the last of September, and at Goschenen they were not entirely ready at the end of December. M. Favre was occupied with other things, but was constrained by the exigencies of the chief engineer to take the matter of completion in hand. To this task were added many others—he was delayed by obstacles still more foreign to his work. Diplomatic difficulties arose in November to occasion the delivery of material at Mt. Cenis, of which the purchase and expense was imposed upon M. Favre. For nearly two months he was kept in suspense without learning what was required or the time for delivery. This resulted in the loss of the autumn months, which were most favorable, and the last half of the winter was visited with more snow than had been known for half a century; thus the work of the contractor and the greater part of his transportations were delayed until May, 1873.

These united circumstances, aggravated by conditions already too severe, gave the contractor a right to claim special indulgence on the part of all interested.

The Italian Government was doubly honorable in dealing with the work at Frejus, to the undertaking itself and in exceptional ways towards those directing the execution—the engineers or contractors.

The Egyptian Government and the subscribers to the Suez Canal imitated this noble example toward M. de Lesseps.

It will be the same with the work at Gothard. The greatness of the task and the efforts exerted already will recommend the enterprise to the kindness of the public and especially to that of Government.

PHOSPHOR-BRONZE AND ITS APPLICATIONS.

By ALEXANDER DICK.

From "Journal of the Society of Arts."

According to antiquarian research, bronze formed the earliest alloy produced—some specimens are estimated to have an existence of from 4,000 to 5,000 years; it was not the alloy which we now call bronze, formed of copper and tin, but the result of the rough smelting of cupreous ores, and, of course, contained many impurities. It was used for casting domestic tools, weapons, and images; at a later period for bells, and still later for cannon.

The difficulty of the bronze founder, during the later periods referred to, appear to consist in the impossibility of obtaining pure copper and tin for his smelting, but even when these became articles of commerce, further difficulties were met with in producing sound and reliable castings, and it is of but a recent date that some light has been thrown on the nature and cause of these difficulties, as also on the way to avoid or remedy them.

In the year 1868, Messrs. Montefiore and Künzel, of Liége, in Belgium, engaged in a series of very exhaustive experiments with bronze and bronze castings.

These gentlemen observed that the tin in bronze continually decreases by oxidation during the process of smelting. They found that the oxide of tin partly goes into the slag and partly is dissolved in the molten metal.

	Tin.	Copper.
Bronze originally composed of..	10.10	+ 89.90
Contained after the 1st smelting	9.82	+ 90.18
" " 2nd "	9.40	+ 90.60
" " 3rd "	9.16	+ 90.84
" " 4th "	8.52	+ 91.48

The operators found great difficulty in determining analytically the proportions of the oxides, whether in combination with the tin or copper, or whether only mixed up in the metal; they found that "poling," a process by which the molten metal is stirred up with a wooden stick, will eliminate the oxide combined with copper, but has no influence on the oxide of tin; experiments, by passing hydrogen over heated bronze filings, proved equally ineffectual; the oxide combined with the copper was converted into water, and weighed as such, but the oxide of tin remained as in the "poling" process. It was in consequence of this difficulty that Dr. Künzel, for the first time, introduced phosphorus into the molten bronze; this had the desired effect, and the metal, which at first had a dull and covered appearance, suddenly became bright and of metallic surface. Thus, by introducing a small proportion of phosphorus, or phosphuret of tin or copper, of previously ascertained composition, it was found that the total amount of oxygen contained in the molten metal could be ascertained analytically with perfect certainty.

The absolute and elastic resistance of old bronze, that is, bronze containing oxides, is much smaller than that of bronze made with new metal; and, as will be seen by the following experiment, "poling" will improve old bronze, whilst phosphorus greatly improves bronze "poled."

Shavings of old bronze was melted and a bar thereof cast at 1595° C., the remaining liquid bronze was stirred with a wooden stick ("poled"), and a second bar cast at 1668° C. The then remaining metal was deoxidised with phosphorus and a bar cast at 1614° C. The three castings were thus made out of the same crucible and cast in the same manner in three moulds. The results were:—

	Resistance. absolute lbs. per sq. in.	elastic lbs. per sq. in.	Lengthening until rupture. Per cent.
Old bronze.....	22,982	17,020	2.0
" poled...	24,922	17,709	2.8
" deoxidised with phosphorus	33,916	19,300	6.8

Thus by the entire reduction (elimination of oxide), the old bronze had tripled its tenacity and considerably augmented its absolute resistance.

Having so far succeeded in eliminating the oxides from the bronze, the inventors showed that by the further addition of a

small per centage of phosporus to the bronze alloy the qualities of the latter became more and more changed, the grain or fracture became finer, the color brighter, the elasticity and resistance to strain and compression considerably increased, and when melted it attained greater fluidity.

Similarly, as minute quantities of carbon alter the physical properties of iron and convert the latter into steel, so a minute quantity of phosphorus changes those of bronze and converts the same into phosphor-bronze. It may thus be said in a certain sense that what steel is to iron phosphor-bronze is to ordinary bronze.

Messrs. Montefiore and Kunzel next experimented with alloys of copper and nickel, and with manganese (binary alloys), also with (ternary) bronzes of copper, tin, and manganese, with copper, tin, and nickel, as well as with iron alloyed with copper and tin.

The manganese alloys they concluded to be entirely useless, as also those of nickel and of iron. They obtained great tensile strength and hardness with some of these compositions, but their ready oxidation at high temperatures made the qualities of the castings uncertain and impracticable.

Sodium eliminated the oxides contained in the molten bronze, but the slightest surplus of it produced an alloy which could not resist the atmospheric influences, and oxidised with great rapidity.

A minute addition of zinc to ordinary bronze augments the resistance to rupture by reducing involved oxides, but it softens the alloy and causes it to lose its elasticity.

Phosphorus was therefore found to be the only ingredient which will improve bronze by giving reliable results.

The action of phosphorus is twofold—firstly, it eliminates the oxides as stated above, and secondly, it makes the tin capable of adopting a crystalline structure—and, as two crystalline metals form a much more homogeneous alloy than two metals, of which one is not crystalline, phosphor-bronze must necessarily be more homogeneous than ordinary bronze. Homogeneity and absence of oxygen increase the elasticity and absolute resistance of the alloy.

Another great advantage of phosphor-bronze is that its hardness can be regulated by varying the proportions of phosphorus, which, in ordinary bronze, is done by increasing the proportion of tin, whereby the danger of segregation in the casting is greatly augmented. Ordinary bronze, after one or two smeltings, becomes thick-flowing, and putty-like, whilst phosphor-bronze remains perfectly liquid until the moment it sets (solidifies), if, therefore, it is cast just before the "setting" takes place, no segregation is possible.

Combinations of phosphorus with copper, with tin, or with other metals, have long been known by chemists, but Dr. Kunzel was the first to employ the same for the purposes above stated.

A number of phosphor-bronze alloys are now manufactured, varying in composition to suit the objects for which they are intended. The scope of their applications is of course very great. The harder alloys are used for casting bells, tools for gunpowder mills, &c.; other somewhat softer alloys are used for engineering purposes, and the still softer for rolling, drawing, and embossing, &c.

The following tables will show the results of tests made by Mr. Kirkaldy, of Southwark Street, with various phosphor-bronze alloys :—

Cast Metal.	Diminution of Section before Rupture.	Resistance in pounds per square inch.	
		Elastic.	Absolute.
	Per Cent.	Pounds.	Pounds.
Pure Copper	3.30	4.4000	6.975
Ordinary Gun Metal, containing 9 parts copper, 1 part tin	3.60	12.800	16.650
Phosphor-Bronze	8.40	23.800	52.625
" "	1.50	24.700	46.100
" "	33.40	16.100	44.448

Drawn Metal.		Pulling Stress per square inch.		Twist in Five Inches.		Ultimate Extension.
		Wire as drawn.	Annealed.	Wire as drawn.	Annealed.	
		lbs.	lbs.			Per Cent.
Various Alloys.	Phosphor-Bronze	102.759	49.354	6.7	89	37.5
	" "	120.957	47.787	22.3	52	34.1
	" "	120.950	53.381	13.0	124	42.4
	" "	139.141	54.111	17.3	53	44.9
	" "	159.515	58.853	13.3	66	46.6
	" "	151.119	64.569	15.8	60	42.8
Copper		63.122	37.002	86.7	96	34.1
Steel		120.976	74.637	42.4	79	10.9
Iron, galvanised best charcoal E.		65.834	46.160	48.0	87	28.0

A series of interesting experiments with phosphor-bronze was made in Berlin by the Royal Academy of Industry, in order to ascertain the qualities and capacities of the metal whilst under heavy strain, and its resistance to often repeated strains. The first bar of phosphor-bronze was tried under a constant strain of 10 tons per square inch, and resisted 408,230 pulls; a bar of ordinary bronze broke even before the strain of 10 tons per square inch had been attained. A second bar of phosphor-bronze was tried under a strain of 12½ tons per square inch, and withstood 147,850 pulls, and a third bar, under 7½ tons strain, broke only after 3,100,000 pulls. On the bending machine, phosphor-bronze, whilst under 9 tons strain per square inch, remained unbroken after 4,000,000 bends, whilst ordinary bronze broke after 150,000 bends.

Major Majendie tested phosphor-bronze as to its liability to emit sparks, when subject to friction, and attained very satisfactory results. The experiments were carried out at the royal gunpowder works at Waltham Abbey. A grindstone of 9 inches diameter was made to revolve very rapidly, so that any point on the grinding face would describe a distance of 2,000 feet per minute; the metal was then pressed against the revolving stone, and the results proved that the harder descriptions of phosphor-bronze emit sparks less readily than the softer samples, and much less readily than ordinary gun-metal or copper.

For frictional purposes the Phosphor-Bronze Company, in London, produces a special alloy by fusing phosphor-bronze in certain proportions, together with another soft alloy of different degree of fusibility, so as to produce by cooling two given alloys. The shell is then formed of a very tough and hard phosphor-bronze and the interior of aforesaid soft alloy. The bearing surface may then be considered to consist of a large number of small bearings of soft metal, enclosed in casings of metal almost as hard as the arbor itself. The microscope reveals this disposition very plainly, and if one of these bearings be carefully submitted to heat, so as to cause the soft fusible metal to run off, the rest will remain in the form of a spongy mass. Bearings, slides, eccentrics, &c., of this peculiar alloy are now very largely in use, and the practical results show that it wears more than five times as long as gun-metal.

Phosphor-bronze is readily rolled or beaten out into sheets. In Russia it has been used as a material for cartridge sheathing, and specimens have stood 120 trials without tearing. Sheets of the alloy stand the action of sea-water much better than copper. In a comparative experiment made at Blankenbergh, lasting over a period of six months, between the best English copper and phosphor-bronze, the following results were arrived at :— (*See Table on following page*).

The loss in weight, therefore, due to the oxidising action of sea-water during the six months' trial, averaged for the English copper 3.058 per cent., while that of the phosphor-bronze was but 1.158 per cent., or about one-third.

Thickness of the Sheets = 0.236 inches.	Weight before Immersion.	Weight after Immersion.	Loss of Weight.	
	lbs.	lbs.	lbs.	Per Cent.
Sheet of Copper.....	74.4	72.2	2.2	3.015
" "	88.9	86.2	2.7	3.100
Sheet of Phosphor-Bronze.....	69.5	68.75	0.75	1.123
" " "	114.3	112.97	1.03	1.195

Several governments have experimented on the use of the alloy for making cannon. Without any exception, the results showed a much greater resisting power over that possessed by ordinary bronze. The following instances of the results arrived at will be of general interest :—

In Belgium the ordinary bronze gun burst at the second shot, with a charge of 1 k. 250 gr. (2¾ lb.) of powder, and a cylindrical projectile weighing 8 k. 518 gr. (18¾ lb.) The phosphor-bronze gun supported this charge perfectly ; the normal charge was 500 gr. (1 1/10 lb.) of powder, and 3 k. (6¾ lb.) of projectile.

In France the ordinary bronze gun burst at the second shot, with a charge of 1 k. 500 gr. (3½ lb.) of powder, and 16 k. (35¼ lb.) of projectile, while the phosphor-bronze gun was fired five times with this charge, and burst at the second shot with 1 k. 750 gr. (3⅞ lb.) of powder, and a projectile of 20 kil. (44 lb.), owing to the wedging of this in the barrel. The normal charge was 0 kil. 550 gr. (1⅜ lb.) powder and a bomb of 4 kil. (8⅘ lb.)

In Prussia it was shown in firing with the regulation charges, and diminishing, at each 50 shots, the exterior diameter of the chamber, that the phosphor-bronze cannon only changed their dimensions when the thickness of the metal was below that of the dimensions of a cannon of the same calibre made of steel.

The Phosphor-Bronze Company now supply most of the English Railways and nearly all the rolling mills, the Royal Arsenal, and many of the leading engineering firms. Amongst the steamship companies the Peninsular and Oriental line has nine steamers fitted with phosphor-bronze in parts of the machinery, the Temperley line, the Wilson line, State line, Messrs. McAndrew & Co., the Ducal line, Globe line, the North Cape Mail Co., Messrs. Pickernells, Messrs. MacIver & Sons, and numerous other firms, as also 25 ships in H. M. Navy are fitted with phosphor-bronze.

In conclusion I beg to draw attention to the specimens of phosphor-bronze exhibited here—they are separated into different groups, each group is headed by an ingot of the phosphor-bronze alloy used for the manufacture of the various articles belonging to that group.

In each group is shown an ingot of the phosphor-bronze alloy used in the manufacture of the respective articles contained in the group ; also test bars from Mr. David Kirkaldy, of Southwark. These tests were made with bars cast in sand, except those of Group D, which was cast in an iron mould and then turned down so that the area of the section corresponded to one square inch exactly. The resistance of each bar is marked upon it, and it is interesting to notice the fractures, elongations, &c., of these bars.

Group A.—Contains the soft and malleable alloys. They can be rolled or drawn, and, like iron or steel, thereby greatly increase their strength and elasticity. These alloys have the appearance of copper which, in fact, is their chief constituent. The tests made with phosphor-bronze wire show that if drawn hard, the tensile strength can surpass that of steel wire, while when annealed, it is much tougher than annealed copper wire, its elongation before rupture takes place being 46.6 per cent. against 34.1 per cent. of copper wire. Amongst the specimens here before us is a piece of phosphor-bronze wire drawn so fine that it takes 2,000 yards for the weight of one ounce. The wire is used for wire webs in paper mills, for pit and other ropes where acid waters occur. For telegraphic purposes a phosphor-bronze wire is made which possesses the same tensile strength as iron wire, and three times its conductivity. Copper wire, which has the greatest power of

conducting electricity, is too soft to be suspended without the support of steel wire; it "bags" between the poles, as it cannot bear its own weight. Phosphor-bronze tubes of various sizes are for use in boilers, &c., and the finer sort for Bourdon's gauges. The sheathing can be made like the wire, either very tough and malleable or hard and springy.

Group B.—This alloy is of about the same hardness as ordinary bronze, but much superior to it in strength, elasticity, color, and fluidity when melted. It possesses a rich, gold color when polished, and the castings have the strength of wrought iron. It can be hammered, but will roll only with difficulty. It is used for steam fittings and harnesses. The Belgian cavalry have phosphor-bronze buckles, and the Garde Civique in Belgium phosphor-bronze gun-actions in use for their Comblain rifles. Messrs. Chubb & Son, and other lockmakers, use it for keys. In engineering works it is used for a great variety of things. On account of its great fluidity in the melted state, this alloy is very suitable for ornamental castings; these leave the moulds so true that but little chasing is required to finish them.

Group C.—The hardness and grain of this alloy approaches that of cast steel; although it will stand a blow with the hammer, it will not improve its elastic and tensile strength by hammering. It is chiefly used for parts of machinery, for pumps, valves, pinions, piston rings, &c.; also for the three-cylinder engine of Messrs. Brotherhood for Whitehead's fish torpedoes. A plunger cast in this alloy remained without any mark of wear after 572 days, where a steel plunger wore out within six weeks. As worm wheels it lasted eighteen months against the same cast in gun-metal lasting but a few weeks. An eccentric strap of this phosphor-bronze alloy wore but ¼ inch in eighteen months, while a similar one in gun-metal only lasted three months. In her Majesty's Navy it is used for slide faces, and the Royal gunpowder-mills at Waltham-abbey, as also many private firms use it for tools.

Group D.—The alloy forming this group is less hard than that of group C.; its grain is very fine, but its main feature is its great tensile strength. The tests show over 26 tons of absolute resistance per square inch. It must be cast in metal moulds, and its chief use is for bolts and nuts, for spindles, pump rods, &c.

Group E.—Bearing metal. The nature of this peculiar alloy has been mentioned already; its applications for rubbing surfaces is of course very manifold, and its use undoubtedly the most extended of all the phosphorized alloys. It is much appreciated for bearings in rolling mills by railways for slides and axle-boxes, and for carriage and loco bearings, for eccentrics, &c., it has given the greatest satisfaction.

Considering the comparatively short time since phosphor-bronze was first known in England, its use is very large—its superior qualities gave it a ready introduction to engineers, and it now proves itself quite an article of necessity with which many branches of industry can dispense no more.

NOTE ON "RECENT EXPERIMENTS ON FLOW OF WATER IN RIVERS AND CANALS."

By Brevet Major-General HENRY L. ABBOT.

Written for Van Nostrand's Magazine.

My attention has been called to a translation, in the June number of your Eclectic Engineering Magazine of an article upon "The Flow of Water in Rivers and Canals," which appeared in *Der Civilingenieur*, from K. R. Bornemann, Kunstmeister at Freiburg, Saxony.

In it appears the following sentence, (p. 536):

"Gordon's experiments, however, inspire a far greater confidence than those of Humphreys and Abbot, for Hagen has shown that it is extremely probable that their results were altered in part to

establish the theory proposed. His velocity measurements are also free from the suspicion attaching to theirs, for his lower float was connected with the upper by a very small cord, while in the Mississippi Survey the surface of the cord for measurements at a depth of 100 feet was 1½ times as great as that of the float itself."

In intimating that in his opinion our results were altered to establish a theory, Mr. Bornemann has exceeded the limit of professional courtesy. Our observations were published in great detail; and as the figures "establish the theory proposed," this language implies a dishonest tampering with the field notes—a suggestion which, whether made by Mr. Bornemann or Mr. Hagen, or by any other person is absolutely false and libelous.

As you have given currency to this statement in your Magazine, I have no doubt you will do the justice to insert this reply in your next issue.

I will add that the remark about the size of the cord used in the Mississippi observations, contained in Mr. Bornemann's article is based upon an error which occurred in the First Edition of the Mississippi Report, and which was officially corrected by Prof. Forshey who made most of the sub-surface velocity observations in question. His letter upon the subject will be found on p. 372 Part II of the report of the Chief of Engineers for 1875.

THE DECENNIAL PERIOD OF MAGNETIC VARIATIONS, AND OF SUN-SPOT FREQUENCY.

By JOHN ALLAN BROUN.

From "Nature."

A CENTURY and a half ago Graham discovered that the north end of a magnetic needle moved from morning till afternoon towards the west, returning thereafter to its most easterly position in the morning again. Van Swinden, who, half a century later, studied this phenomenon during several years, occupied himself greatly with the deviations from the diurnal law. One of these, the occurrence of the greatest westerly position before noon or after 4 P.M., he found to happen most frequently in 1776, the number of times increasing from 1772, and diminishing from the year of maximum till 1780. He then asked the question whether there was not a period of *eight years*. Van Swinden's results were greatly affected by imperfections of his instrument, and we can only consider that the excess of irregular days in 1776 was probably chiefly due to real causes.

Though several series of magnetic observations were made during the eighteeenth century, and two series early in this (those of Beaufoy and Arago), yet, as far as I can discover, Kaemtz seems (in 1836) to have been the first to remark that the mean value of the diurnal oscillation of the magnetic needle was not constant, but varied from year to year: this conclusion he founded on Cassini's observations, which gave the mean oscillation 9′.71 in 1784, and 15′.10 in 1787. The illustrious Gauss drew more distinct attention to the fact, for, in studying the observations made at Gottingen in the years 1834 to 1837, he pointed out that the mean diurnal oscillation for each month in the second year was greater than that for the corresponding month of the first year; and that a similar increase was to be found in the third year compared with the second. This increase Gauss did not think could go on long, and he predicted that by continuing the observations for several years, an oscillation in the mean value would present itself. It is not a little curious that in discussing the Gottingen observations for the next three years, Dr. Goldschmidt should have failed to remark that the maximum was attained in 1837, and that thereafter the mean diurnal oscillation was diminishing. This was reserved for Dr. Lamont, the distinguished astronomer of Munich,

who, in the end of 1845, by adding the mean oscillations obtained from his own observations in 1842-1845, to those already found for the preceding years at Gottingen, was able to state that the minimum was then attained, but that a longer series of observations was required, in order to determine the law of the oscillation.

It was only in the end of 1851, when the maximum oscillation (which occurred in 1848-49) was decidedly past, and the mean oscillation had again begun to diminish in value, that Dr. Lamont published his conclusion that the diurnal oscillation of magnetic declination (as well as of magnetic force) obeyed a law whose mean duration was nearly $10\frac{1}{3}$ years. For the determination of this mean he employed the epoch of maximum oscillations shown by Cassini's observations in 1787 (already noticed by Kaemtz), and he assumed that there were six periods from that date till 1849.

Schwabe had previously, from his persevering observations of the number of spots on the sun's surface, arrived at the conclusion that these obeyed a decennial law, so that the number was a maximum in 1828, 1837, and 1848, while it was a minimum in 1833 and 1843. The agreement of the epochs, 1843 and 1848, with those of minimum and maximum magnetic disturbance deduced by Sir E. Sabine from the observations made in the colonial observatories, was at once remarked by him, as well as that of Lamont's epochs with those of Schwabe.

This coincidence was also immediately afterwards, and quite independently, brought to public notice by Dr. Wolf, of Bern (now of Zurich), and M. Gautier, of Geneva. It is, however, with the important labors of the former of these philosophers that we are most concerned. Dr. Wolf began at once a systematic search for observations of sun-spots, and examined hundreds of volumes printed and in manuscript, dating from the first discovery of the existence of spots on the sun's surface. All the observations thus accumulated he has endeavoured to connect and to reduce to a common unit; and from the numbers thus obtained he has concluded that the sun-spot period, as well as that of the magnetic variations, occupies on the average $11\frac{1}{9}$ years.

One great cause of the difference between the results of the Munich and Zurich astronomers is to be found in the interval 1787 to 1818. According to the former, three periods *ought* to have occurred in this interval; according to the latter, only one maximum happened, *in fact*, between the two of 1787 and 1818. Dr. Wolf has concluded, from the magnetic observations of Gilpin (1786-1806), that a minimum of the diurnal oscillation of the magnetic needle occurred in 1796, and a maximum in 1803, and these epochs he has supported by the observations of the numbers of sun-spots, as well as of those of the aurora borealis, a phenomenon known to be associated with magnetic disturbance, and to have the same epochs of frequency. On the other hand, Dr. Lamont has maintained that Gilpin's observations are without value, as his needle was supported on a steel pivot, and sometimes did not move freely; he has also objected to the observations of sun-spot frequency made during the time in question, that they were made rarely, without any common system, and by few observers, some having at times seen no spots when others saw many.

If we could assume with the astronomer of Munich that Gilpin's observations and those of sun-spot and auroral frequency made at the same time are worthless, all our knowledge of the epochs of magnetic oscillations since 1818, and of sun-spot frequency since 1826, would induce us to conclude that there were really three periods during the thirty-one years 1787—1818. If, however, any value can be given to the observations during that interval, it is not allowable to assume that the durations of the periods have always been the same, the more especially that we know the period has varied in length from eight to twelve years within the last half century. That some value is due to observations of three different phenomena has been allowed by most writers, and Dr. Wolf's period of $11\frac{1}{9}$ years has, in consequence, been accepted by many of the most eminent men of science who have had occasion to allude to the subject.

Having had to study this question in connection with the results of observations made during twenty-three years at Trevandrum, I have examined with care the magnetic observations of the last

and the present century, determined the exact times for which the yearly mean diurnal oscillation of the magnetic needle was a maximum or minimum, and have arrived at the following conclusions:

1st. That there are not sufficient grounds for rejecting the observations of Gilpin, which appear to be in general trustworthy as regards the change of mean position of the needle from year to year, and of the diurnal range from winter to summer.

2d. That these observations should, according to the mean law, show a maximum near 1797, and another should have occurred near 1807. I have found that they do indicate a maximum in the former year; and though another maximum appears in 1803, that there are grounds for believing the maximum may really have occurred after 1806, when Gilpin's series terminated.

It has to be stated, however, that the maximum shown by Gilpin's observations in 1797 is very small; that the whole interval between the preceding and following minimum is not six years; and that no such short period and small minimum have been observed during the last half century. Since, however, the shortness of the period and the smallness of the maximum are both confirmed by the observations known to us of the frequency of sun-spots and of the aurora borealis, I can only conclude, in conformity with the facts, that both these were real phenomena, which may yet be repeated and aid in the determination of the cause of the decennial period. The mean duration of the period at which I arrive is therefore almost exactly that which Dr. Lamont had previously obtained, or 10.45 years.

For this result the facts have been taken as they present themselves; since it would be difficult to conclude that the observers of all the three phenomena could have erred in the same way during nearly twenty years. In addition to this, after a careful study of Dr. Wolf's sun-spot numbers, I find it impossible to accept his period of 11⅛ years. How ill the facts satisfy this result may be shown by two comparisons in which the epochs accepted by the Zurich astronomer are employed.

Thus a maximum of the magnetic oscillation occurred in 1787 by the observations of Cassini and Gilpin; this epoch has been confirmed nearly by Dr. Wolf's sun-spot numbers, and by Prof. Loomis for the auroral frequency. We have then the last observed maximum 1870.9, about which there can be no doubt. In the interval between these two maxima there were, according to Dr. Wolf, only seven periods, consequently we have—

$$\frac{1870.9-1787.3}{7}=\frac{83.6}{7}=11.94 \text{ years},$$

a period which differs as much from his mean period as that does from Dr. Lamont's. If on the other hand we take one of Dr. Wolf's sun-spot epochs about eighty years before 1787, and employ the number of periods he has himself given for the interval, we find—

$$\frac{1787.3-1705.5}{8}=\frac{81.8}{8}=10.23 \text{ years}.$$

If, then, we commence with the epoch of 1787 and compare it with any epoch of maximum since, we shall always find for the mean duration at the least 11.9 years according to Dr. Wolf; and if we compare it with any of the epochs given by him upwards of eighty years before, we shall never find a greater mean than 10.75 years, and this result includes an interval of 172 years before 1787, with all the uncertainty of the earlier epochs. This great difference of more than one year in the mean duration, as derived from eighty-four years after 1787, and eighty-two to 172 years before, disappears to a great extent if we admit three periods between 1787 and 1818.

It has been already remarked that the duration of a period is not constant, but varies within certain limits. The question naturally presents itself—does this variation follow any law, or is it accidental, increasing one year and diminishing the next? The number of periods for which we have the epochs of maxima and minima of the diurnal oscillation of the magnetic needle accurately determined, is not sufficient for any very sure reply. At the same time the results I have obtained indicate a period of nearly forty-two years for the repetition of the variations in question; and if this conclusion is confirmed by next maximum, that should occur in the year 1879. It may also be pointed out that accord-

ing to the law of forty-two years a maximum should have occurred in 1818—42 =1776. Now this year, according to Dr. Wolf, was a year of minimum. The variation of his sun-spot numbers for that period, it appears to me, is not sufficient to give his conclusion much weight; while, on the other hand, Van Swinden's result, which it is extremely probable was a consequence of the decennial law, gives 1776 for the year of maximum; and that it was so is further supported by the magnetic observations of Cotte, at Montmorency. The exceptional period about 1797 shows, however, that any definite conclusion from observations during the last sixty years may be impossible, since causes of variation exist with which we are insufficiently acquainted as yet.

When we compare the mean range of the diurnal oscillation of the needle for the year in which it is a maximum with that for the year of minimum at any station, we find that the ratio of the two is very nearly constant for places so widely separated as Toronto, Dublin, Trevandrum, and Hobarton. I have also found that the *law* of the diurnal movement is the same in the year for which the range is least, and in that for which it is greatest. This shows that it is the same cause which is acting, the variation being one of intensity only. Since few or no sun-spots are visible in the years of minimum range, we perceive that the sun-spots happen only when the intensity of the force producing the magnetic variations exceeds a given value. It also appears that considerable variations in the amount of magnetic disturbance may exist near the equator when there are few or no sun-spots; and, on the other hand, that the spotted surface of the sun may be a maximum, and no corresponding increase of the magnetic oscillations be visible. The latter are, however, exceptional cases, since increases of sun-spots and of magnetic movements occur frequently near the same time; the increase of the one, however, bears no constant proportion to that of the other.

It has been already stated that the ratio of the diurnal oscillation of the needle in the year of maximum to that in the year of minimum is very nearly constant for places very widely separated from each other; there are, however, slight variations in the ratio shown at some places; thus, although it is nearly the same at Toronto, Dublin, Trevandrum, and Hobarton (1.55), it is slightly greater for Munich and Lisbon (1.71). This is probably due to the action of disturbances which are known to obey local laws. I have also found for Trevandrum, nearly on the magnetic equator, that the disturbances, or the deviations of the magnetic needle from the mean position, do not show exactly the same epochs of maximum and minimum in the decennial period when different hours are considered. Thus, though the cause is cosmic, the actions appear to be influenced, though but slightly, by circumstances of locality.

When we seek for the cause of the decennial period, we are met at first by the three phenomena which obey this law: the magnetic variations, the sun-spots, and the aurora borealis. The connection between the first and third is so marked, that if a magnetic disturbance commences during the day in a high latitude, it is quite certain that the aurora will be seen as soon as the disappearance of sunlight permits. This is a fact I have verified during several years' observations in the south of Scotland. Both these phenomena are results of electrical motions. It did not seem improbable then that the solar spots might be connected with disturbances of electrical equilibrium, and that these might be due to the different electrical states of the sun and of the planets.

We do not know, however, of any planet with a period of ten and a half years, nor of any combination of planetary positions which would produce such a period. My own researches have failed in connecting the variations of the sun's spotted surface with the time of revolution of any planet by a law which holds for different decennial periods. This fact, however, does not disprove a planetary action. We are unacquainted with the nature of the medium through which the electrical actions producing the magnetic variations are conveyed. Physicists seek to reduce the phenomena of nature to the fewest possible factors: many then have been induced to believe that electrical and magnetical actions are conveyed by the same ethereal

medium which we believe transmits heat and light. The facts do not appear to be easily explained by such a hypothesis; thus I have found that certain electrical actions of the sun producing marked diminutions of the earth's magnetic force happen exactly at successive intervals of twenty-six days; when one point of meridian of the sun returns to the same position relatively to the earth; this action, similar to that of a beam of light reflected from a revolving mirror, which illuminates a particular point only at the same part of its revolution, has no resemblance to that of light and heat, which are propagated equally in all directions.

If, then, we can suppose, that the electrical medium is disposed unsymmetrically around the sun, that the disposition and extension varies, it is obvious that the supposed planetary actions would also vary, and might be quite different for different parts of their orbits, in different decennial periods. This suggestion may explain why I have not been able to find a law remaining the same in the different periods; and it is not opposed to the conclusions of Messrs. De la Rue, Stewart, and Loewy, who have found very remarkable relations between certain positions of the planets and the amount of the sun's spotted surface during a single decennial period.

Any hypothesis which seeks to explain the mode of production of the sun-spots (by cyclones or otherwise) must also explain why the causes become insufficient for their production every ten and a half years.

M. Faye, the distinguished French astronomer, considers that the prime cause of sun-spots is to be found in the excess of heat radiated; so that the spots are the symptoms of a dying sun; that we have in fact here a phenomenon like the flickering of an expiring lamp which may have a periodical character. Such a hypothesis will scarcely satisfy the demands of science, but we must evidently wait for more facts before any satisfactory theory can be proposed.

DAMP HOUSES.

From "The Architect."

At the close, as we are now fain to hope, of a winter, which has not only been the wettest within the memory of man, but which, according to all accounts, has swum over the land in almost double the average depth of rainfall, there may perhaps be a good many people with whom the question of domestic damp is one of considerable melancholy interest. With one it is the rain that has "come through the ceilings." With another it is the same rain that has "driven right through the walls." Here the water has risen in the cellars, or in the "kitchens," until trim waiting maids have had to navigate them in a tub. There the mere "damp" has proved almost more unmanageable than any amount of palpable wet, by creeping up the walls of the rooms, or making the carpets and floorcloth as mouldy as old cheese, or actually procuring the importation, from some mysterious source under the floors, of fungi by the square yard, or by the pound, or by the bunch of mushrooms bursting playfully into the light through the crevices and crannies of the skirtings. If, therefore, we submit a few observations of a practical character upon some of these miserable manifestations of water in the wrong place, it is not at all unlikely that some of the sufferers may take heart of grace for some other wet winter, by reason of the acquirement of a little elementary knowledge upon the unpleasant but solemn subject.

When a damp spot appears in a bedroom ceiling after a shower of rain, the cause is generally too simple for any elaborate explanation. There is a slate or a tile cracked or displaced; and it must be repaired, and perhaps the whole roof examined. But at any rate we may take occasion to allude to the well-known complaint that when the slater mends

one slate he breaks three. There is a very intelligible fact at the bottom of this. A roof slate is the most brittle of all building materials—except of course glass. It has to be fixed in its place with two nails. If it should be fixed in any degree awry—and it is ten to one that it is—sooner or later the slate becomes cracked. This may be by frost, or even by wind; but it is obviously much the most likely to occur by the passage of a man's weight over the surface; and hence the mending of one slate may cost the breaking of three dozen as easily as of three, if the slater be only sufficiently painstaking in his examination.

Where snow drives in through the crevices of the slating, this is only to be remedied by pointing or stopping up all those crevices from within—a thing which of course ought to have been done at the first in any circumstances wherein it must require to be done at the last.

The case of wet coming through the solid wall is a very common one. The rain is dashed against the outer surface by the wind; the water clings to the surface; the wind is simply so much pressure per square foot brought to bear upon this water; and if the porousness of the wall material—stone or brick—happens to be such as to allow this pressure to force the water through its whole thickness, the wet must of course appear inside. A south-west aspect in an exposed situation in the country, with a sandstone wall, or with a pretty red-brick facing, is eminently favorable for this transmission of the wet; and indeed a strong wind will carry it sometimes straight through as much as eighteen inches in thickness of solid brickwork, or a good deal more of solid stonework—one of the most experienced of masons going so far as to say that he knew no stone in the British Islands (granite not being overlooked) which would hesitate to carry the damp through a thickness of three feet if fairly put to the test. Constructive precautions against this kind of damp-plague are very well understood and constantly put in practice. To form a hollow wall is the commonest plan. To line the inside of a stone wall, whether hollow or not, with hard brick is a thing often done. To avoid in every possible way the carrying of header stones through the whole thickness of the wall is a self-evident rule. To batten and lath the inside, instead of plastering directly on the stone or brick, is most effectual. But it must be borne in mind that this battening—which in fact leaves an inch or so between the wall and the plastering—only conceals the damp after all; for even if it be nothing more serious than a case of the mere absorption of passive moisture by soft stone, whatever amount of wet actually comes through to the inner surface is obviously left there behind the lathing, to evaporate as it best can.

To remedy the defects of a wall of this class is now an easy task. The ends of header-stones may have to be cut away and patched; or the entire surface may have to be re-plastered on battening and lath; or some kind of paint or paper or the like which is impervious to water may be applied to the surface—although it is difficult to see how anything of this kind can be really effective except in cases of a very slight moisture, affecting for instance the wall-paper and no more. It may be fairly alleged, however, that the application of certain washes or paints externally may prove much more serviceable. In this instance the first access of the damp may possibly be prevented, whereas in the other it is only its exit that is attempted to be stopped.

Where the basement rooms become flooded, this is manifestly due to some one of half-a-dozen causes of an altogether different character which it would be impossible here to discuss. One of these causes, however, may be alluded to as being exceptional in its nature and in its effects, namely, the rising of underground water at almost unexpected times. This occurs generally in marly or gravelly soils where there is a substratum of clay. This surface-bed is connected, perhaps at a considerable distance, with similar soil at a higher level, the clay substratum extending from the one locality to the other. The result is, after rain has fallen heavily on the remote higher level, that it finds its way along the back of the under-clay to the lower level and then breaks out in the form of land springs. If this be, as it probably will be, an incurable evil (unless one is

prepared to make the basement a waterproof tank), there may at least be some sort of dismal satisfaction in understanding the reason of it.

The ascent of damp from the ground through the substance of walls is far too common both in town and country. The process of it is easily described. The foundation of the wall being built of some soft quality of brick or stone, the water will rise directly through this material by capillary attraction if encouraged. Consequently when the soil comes to be wet below, the warmth of the house within is quite sufficient to encourage the water so to rise. The common preventative is to introduce in the substance of the wall during the course of building a layer of some damp-proof material, such as slates or asphalte. This keeps back the ascent of the moisture and leaves the foundation alone saturated. But it is plain that if this simple measure be not carried out at the first it is generally almost impossible, or quite so, to adopt it as a remedy after the defect appears; and all that can be said is that, unless the expense can be encountered for inserting a layer of slates by the laborious process of cutting out the stone or brickwork foot by foot, there is perhaps nothing else to be done but to try the makeshift of applying impervious paint, or metallic cement, or whatever other nostrum may offer.

When damp is found to ascend into the house from the whole surface of the soil under it, that is to say, probably through the floors of the basement, this is not difficult to understand. The soil being charged, as matter of course, with atmospheric air, when the temperature without is lower than within—as in the night-time—the cool external air necessarily forces its way into the warm house by every means of access, and, amongst the rest, through the subsoil. If, therefore, this subsoil be damp, the consequence may be that a mouldy smell will manifest itself within the house by reason of the ascent of air from the ground, and it is more than probable that this smell will also indicate the presence of foul gas as well as mere water. The remedy for all this is to take up the floor and make an impervious bed or surface underneath as a covering for the soil. Concrete is no doubt the most handy material for this purpose, but it ought to be good or it may fail to stop either the moisture or the gas.

Our last subject shall be that which is commonly called dry-rot. This is a species, or rather any one of several species, of fungus, which, when established on any surface of wood, runs with rapidity over the whole, and at the same time throws its filaments into the substance. A piece of wood thus attacked by dry-rot soon becomes coated over with either a delicate fibrous tissue or a coarse leather-like skin, and is consumed throughout by what is very expressly called by the name it bears—rot. The plant itself happens to be especially fostered by a combination of damp and warmth, and still more signally when these combined conditions exist without the presence of ventilation. Logs of American pine, for instance, when taken from the hold of a vessel, are not unfrequently found to be affected more or less with dry-rot, and it is well known that the most serious standard example is when the fungus attacks the timber of a ship for want of air. As a theoretical principle, the provision of ventilation effectually prevents the disease from being initiated; and as a general rule, it is even asserted that the subsequent admission of air will check its progress, and indeed cure it so far as it happens to be curable. The fungus, in a word, dies in fresh air. When, however, it has come to any head before discovery, it is scarcely necessary to say that the floor must be taken up, and the skirtings and other parts removed, and every infected portion cut away carefully for the substitution of a sound wood. It is also desirable to apply a wash of some such liquid as diluted carbolic acid. Beyond all other things, a circulation of air must be set up by the insertion of air-bricks or whatever else under the floor, so that, whether the air will kill the fungus or not, it may at least prevent it from spreading.

No doubt there are other forms of the domestic damp disease which one and another reader may be able to recall to recollection from wretched experience, but probably these are the chief. At any rate the man is always to be pitied who lives in a damp house—unless he be some hardy amphibious person who can

afford to say that he disregards such trifles.

Into the details of what diseases of the body—and of the mind—an average Englishman becomes liable by dwelling in damp we do not desire to enter; the discomfort of the thing or even the objectionable appearance of it, is enough for our purpose, and we are certainly offering good advice to every one who has this to complain of when we say that he ought to get at the root of it as soon as he can, and, in so far as it is possible, make an end of it.

CO-ORDINATE SURVEYING.

By HENRY F. WALLING, C. E.

Transactions of American Society of Civil Engineers.

Relation of Surveying to Geodesy.—Surveying is commonly defined as the art of measuring, laying out and dividing land. The similar but more comprehensive art of geodesy applies to the earth itself, as indicated by the etymology of the word, which is derived from $\gamma\eta$, *the earth*, and $\delta\alpha\iota\omega$, *I divide*. Its objects are, the determination of the form and dimensions of the earth and of its different portions; the continents and islands with their lakes, rivers, mountains, civil divisions, etc. Its processes, like those of astronomy, upon which, indeed, it is to a considerable extent dependent, require instruments of a high degree of precision and mathematical computations of great refinement.

General geodetic Co-ordinate System.—In the operations of the great geodetic surveys of the world, including for our own country those of the United States Coast Survey, positions are finally determined by referring them to co-ordinate planes. The plane of the earth's equator forms one of these co-ordinate planes and that of an assumed standard meridian another. If we should assume that the earth is spherical in form, with continental elevations, the position of any geodetical point could be fixed by determining the direction in space of its radius vector, as referred to the two co-ordinate planes, and the length of this radius vector, the origin being at the centre of the earth. The angle with the equatorial plane made on either side by the radius vector would be the latitude; the angle made by its projection upon the equatorial plane with the meridional plane, measured in either direction around a semicircle would be the longitude, and the length of the radius vector, or rather its excess over that to the level of the sea would be the altitude of the place determined. Owing, however, to the spheroidal form of the earth, latitudes as observed and established do not exactly represent the co-ordinate angles here described. The actual latitude of a place is the angle made by a normal to the earth's surface at that point with the equatorial plane. The geocentric angle could easily be computed from the latitude if the earth were an ellipsoid of revolution of known eccentricity.

Irregular Form of the Earth.—But it has been found that this is not the true form of the earth. The equator, and probably all parallels of latitude, taken at the level of the sea, vary more or less from true circles, indicating a want of homogeneity in the materials which make up the earth's volume. Longitudes are measured by noting the earth's rotation angles on the undoubted assumption that its angular velocity is strictly uniform. By noting the difference in time between the passage of a normal to the earth's surface at any particular point, and of another normal at any point on the standard meridian, across a celestial meridian, we obtain an angle which is called the longitude of the former point, the longitude of the standard meridian being zero. Irregularities in the form of the earth of course change the direction of normals to its surface, and the co-ordinate system of Latitudes and Longitudes, used in Geodesy and Navigation, is correspondingly irregular.

Its Determination a difficult Problem.—In consequence of these irregu-

larities, the problem of determining the exact form of the earth is an exceedingly difficult one. Instruments of the highest degree of precision must be used by skilled observers, and the combined results of a vast number of careful observations over widely extended areas must be subjected to the most refined mathematical investigation before its full solution can even be approximately obtained.

Progress made in its Solution.—Its investigation has been going on, however, for one or two centuries, and very fair progress has been made. The great national surveys of the world have been conducted by men eminently qualified for the task, and the results of these surveys, so far as completed, seem to approach quite near to the attainable limits of accuracy.

United States Coast Survey.—This is particularly the case with our own Coast Survey, which is probably unsurpassed, if not unequalled in precision and general accuracy. Our country, however, has thus far failed to realize some very important advantages which might be derived from these Coast Survey operations, although they have been sufficiently carried out to render them available over considerable portions of the country.

Object of this Essay.—It is the object of this paper to point out a simple method by which the high degree of precision which accompanies the Coast Survey work may be made available in the ordinary operations of land surveyors and civil engineers in those districts over which the Coast Survey triangulations have been carried, and at the same time to call attention to the importance of an extension of these triangulations over the entire country, either by the general or by State governments. One of the disadvantages of our peculiar confederate form of popular government is an apparent inability or indisposition to undertake works of acknowledged and eminent utility, unless they are popular with the masses or with those who control the machinery of elections. It is certainly the experience of foreign countries, including several less wealthy and prosperous than our own, that these surveys are many times more valuable than their cost, in the aid they afford to the carrying out of internal improvements, to the equalization of taxes, and to many of the necessary operations of general and municipal governments, as well as of private individuals. But in this country where legislation usually follows, instead of leading, the expression of general public opinion, such works are likely to await the slow and gradual cultivation of the popular mind to a proper appreciation of their great utility.

Congressional Legislation.—In the meantime, it must be admitted that Congress has enacted a very liberal and wise law by which the Coast Survey is authorized to extend its triangulation over any State in which scientific surveys have been provided for by the State Legislature. Moreover the Superintendent of the Coast Survey evinces a disposition to construe this act with great liberality. It is understood that he will, where thorough topographical surveys are undertaken by State authority, cause the subsidiary triangulation to be carried to any reasonable degree of minuteness with the same refined accuracy which has characterized the work already done by the officers of the survey. Indeed, with their elaborate determination of local irregularities in the form of the earth, and their well organized corps of skilled observers and computers, they have an immense advantage, which could hardly be soon attained by any new organization, for extending the triangulation over whole States and carrying it to the secondary and tertiary stages.

Triangulation of Massachusetts.—The State of Massachusetts is entitled to the credit of being the first of the United States to recognize the importance of having her territory carefully surveyed and to commence upon such a work. More than forty years ago, a triangulation was commenced which was completed in 1842. This triangulation will compare favorably with the Coast Survey work and with the other geodetic surveys of the world. But a surprising indifference to its value, or potential utility, seems to have prevailed, and up to the present time, no further use has been made of it than to adopt it for the basis of such State and county maps as have from time to time been published. These maps give only the horizontal

locations of objects obtained from such imperfect surveys as could be paid for by the sale of maps, published entirely by private enterprise, no assistance being given by the State.

STATE SCIENTIFIC SURVEYS.—At the present time, the subject of scientific surveys is being agitated in the States of Massachusetts, Rhode Island, Connecticut and New York. The latter State appropriated $20,000, in 1876, for preliminary organization, to effect which it appointed a skilled director. The other States mentioned have organized commissions to investigate and report on the subject.

REFORM NEEDED IN LAND SURVEYING.—Before proceeding to describe the proposed combination of surveying with geodesy, a brief consideration may be permitted of the urgent need of reform in the prevalent methods of land surveying and of writing descriptions in conveyances of land, based, as many of these descriptions necessarily are, upon imperfect surveys, and frequently upon no surveys at all. Lawyers and land surveyors are perhaps most familiar with the short-comings of these conveyances in failing properly to describe the property conveyed. A large share of the litigation of the country arises from this cause. Indeed the laxity in this respect is something almost incredible. Scarcely one deed of conveyance in a hundred will be found to contain such a description of the land conveyed as would fix its location with certainty, if the fences, walls or other inclosures should become obliterated, a contingency which is quite likely to arise.

CONFLAGRATION OF DETROIT.—An occurrence of this kind on a rather extensive scale took place in 1805, when the city of Detroit was devastated by fire, and so thoroughly destroyed, that it was found quite impossible to ascertain the former boundaries of estates. The restoration was entrusted to the Governor of the State and a council of judges, who could find no better way out of the difficulty than by re-laying out the city on an entirely new plan, dividing the lots among the former owners as equitably as possible.

Notwithstanding this experience, the lines of streets and lots in Detroit are now so uncertain that disputes and litigation in regard to them are of continual occurrence. The same is true in most of the cities and large towns of the United States, especially in suburban districts and growing villages where land is rapidly increasing in value.

FAULTY DESCRIPTIONS IN LAND CONVEYANCES.—If we examine the descriptions given in land conveyances, we shall find that they usually fail to fix either the location of the property by references to permanent land marks, or even the position of its boundary lines relative to each other. Frequently the tract conveyed will be bounded in the deed by the several tracts adjoining, of which the only description given is to state the names of the supposed owners. In many deeds, all dimensions are omitted and only an indefinite approximation to the quantity of land conveyed is given, the statement being that it contains about so many acres, "be the same more or less." Where good permanent division fences, walls, hedges, ditches, streams, shore lines, bound-stones, stakes, &c., mark the boundaries, and are properly described, such descriptions may answer the purpose so long as the boundaries remain unchanged, although such indefiniteness as to quantity would hardly be tolerated in the sale of other kinds of property.

OBLITERATION OF MONUMENTS.—But physical monuments are continually becoming obliterated even when well defined at first. It is said to be an old custom in some parts of the country, to take children once in every year to important boundary corners, where monuments have been erected, to the location and surroundings of which the careful attention of the children is directed. If on a subsequent visit, their memory is found to be at fault, it is refreshed and deepened by combining with it that of a sound flogging.

UNRELIABILITY OF SURVEYS.—Even where surveys have been made, they are in many cases so unreliable that the recollection of old residents in the vicinity, considerately stimulated, perhaps in their youth in the way described, is more reliable in determining the proper location of lost boundaries, than the retracing of old surveys. This is not surprising when the modes of surveying and the

character of the instruments used are taken into consideration.

Outside of cities and larger towns, the instruments usually employed in surveying are the chain and compass. The method is to perambulate the boundary line of the tract to be surveyed, taking the magnetic bearing with the compass, and measuring with the chain the lengths of each side of the polygon forming the boundary.

Imperfection of the Compass.—Now, provided the bearings thus taken were precisely measured angles from fixed parallel meridians, whose directions could always be easily ascertained, when a re-survey should be needed, no better method of noting directions could be desired. But this is far from being true. The magnetic force to which the direction assumed by the needle is due, is quite irregular in its action, changing its direction continually, backwards and forwards, even during the different hours of the day, while larger oscillations extending sometimes to many degrees of arc take place in irregular cycles, perhaps one or two centuries in duration. And this is not all. In regions containing metallic deposits, especially magnetic iron ore, very irregular and powerful local disturbances of the magnetic force arise, causing the needle to take widely different directions, even at points in close proximity to each other, thus destroying parallelism of action, and rendering the compass quite useless for ascertaining true directions.

Beside the uncertainty of the magnetic meridian, there is an incapacity of precision in the use of the compass for measuring angles. The needle must swing clear of the graduated limb, and cannot be suspended for field use, with very great delicacy. In practice it is usually impossible to read a magnetic bearing with greater precision than to the nearest ten minutes of arc.

Theodolite and Surveyor's Transit.—The theodolite and the surveyor's transit are instruments of far greater precision and are generally used in cities, and for more valuable farm lands, also for engineering works, roads, railroads, &c. Those in ordinary use, measure angles to single minutes.

In land surveying, the common method of using these instruments is to measure the angles formed by the sides of the bounding polygon with each other, and sometimes, for verification, with one or more diagonals. The compass needle, usually attached, affords an approximate means of ascertaining azimuths, but with no more precision than with the ordinary compass. It is accordingly, quite as difficult to retrace obliterated boundaries with these instruments as with the compass, unless one or more well-defined lines remain for reference.

Solar Compass.—Burt's solar compass now used in the surveys of the public lands at the West, for running out the parallel and range lines is a great improvement upon the magnetic compass in the accuracy of its azimuths if not in the precision of reading minute angles. The direction of the sun, with proper adjustment of the instrument for the latitude of the place and declination of the sun and hour of the day, affords of course, a reliable means of obtaining the true azimuth of an observed line with as much precision as the mechanical construction of the instrument permits. The use of the solar compass, however, is limited to sunshining or slightly cloudy days, the middle portions of which, moreover, are unfavorable to accurate observations, and at best, the precision of its angular measurements is much inferior to that attainable with the transit. Nevertheless, for the preliminary surveys of wild lands, where no trigonometrical survey has been made, and where rapidity and economy are required, as in the government surveys of western lands, it is a very convenient and useful instrument.

Measurement of Distances.—The direct measurement of distances is attended with even more difficulty than that of the determination of directions. It is accomplished by the repeated application of the chain, tape or measuring rod, as nearly as possible, upon the line to be measured. Passing over inaccuracies in the length of the measuring standard arising from imperfections of construction, inequalities of temperature, changes of length by stretching, kinking, &c., the difficulties of making the direct applications are frequently quite serious. It often happens that access to all parts of the line to be measured is difficult, if not impossible. In fact, the boundaries

of improved properties are generally indicated by walls, fences, hedges, ditches, etc., which occupy a considerable width upon the ground, being partly upon either side of the dividing line, upon which it therefore becomes impossible to apply the measuring standard. In such cases, it is usual to measure the opposite sides of an imaginary parallelogram, equal offsets being made at the ends of the line, and the measurements effected between the offset points. The offset angles are quite frequently estimated by the eye, and brought as nearly as possible to right angles. Even if these angles are instrumentally measured, the operation becomes complicated, thus increasing the liability to error. Impassable obstacles along boundary lines are of frequent occurrence, and various expedients, more or less complicated, are resorted to for ascertaining the distances through them. The difficulties of measuring accurately over uneven ground, requiring a careful and laborious use of the plumb line, the avoidance of sagging or unequal stretching when the chain is used, the exact marking on the ground of the end of the measuring standard, &c., are familiar to all land surveyors.

Measurements of Angles.—It is easy to see that measurements of angles with good instruments can be performed with far greater ease and precision than the measurements of distances by the ordinary methods. Either mode of measurement is merely a determination of *ratios*. Thus, when we measure the length of a line by direct applications of a standard length upon the ground, we simply ascertain the ratio between the length of the measured line and that of the chain. So, if we measure the three angles of a triangle, we know by a simple computation, the ratio of its three sides to each other. The percentage of error in the ratios, as found by either method, is much less in the measurement of angles with good instruments; for although the distances which are compared together in this measurement, namely those marked in degrees along the limb of the instrument used, are many times smaller than those compared together in the measurement of distances upon the ground, the nicety of the mechanism and the ease of the verification by repetition admit a precision quite unattainable in actual ground measurements, except by slow and laborious processes.

Trigonometrical Survey.—In the trigonometrical survey, this superiority of angle measurement is practically recognized. Convenient points are selected, at suitable distances from each other, where the angles between imaginary lines, joining adjacent points, can be measured. The whole area to be surveyed is cut up by these lines into a network of triangles, and the ratios of the sides of these triangles to each other are determined by measuring the angles between them. Then, when we ascertain the length of any one side of any one triangle, we can compute all the sides of every triangle, or the distances from point to point throughout the whole survey.

Degree of Accuracy Attained.—The accuracy of the result, accordingly, depends upon the precision with which this one side or base line is measured, as well as upon the accuracy of the angle measurements. It is usual to verify the whole of the work by measuring another base line in a distant part of the network of triangles. For example, in the Coast Survey work, a base line was measured at Fire Island, on the south side of Long Island, in 1834 ; another in Attleborough, and Sharon, Massachusetts, in 1844 ; and another near the village of Epping, in Columbia, Washington county, Maine, in 1857. The distance between the Fire Island and Massachusetts bases, along the axis of triangulation, was 230 miles, and between the Massachusetts and Epping bases 295 miles. The length of the Fire Island base, 8715.942 metres, or 5.415 miles, as actually measured, varies from the length, as computed from either of the other two bases, by less than 0.07 metres, or 2.75 inches ; and the probable error of any computed line between these two bases is shown by careful analysis not to exceed $\frac{1}{288000}$ of its entire length. This proportion of error to distance amounts to 0.22 inches in a mile, or a little less than 2 feet in 100 miles.

This degree of accuracy indicates the wonderful skill which has been attained in the construction and use of instruments both for the measurement of base lines and of angles. Thus, if we compare the actual distance upon the limb of

a theodolite, 30 inches in diameter, which corresponds to an error of $\frac{1}{288000}$, which we will suppose to be all thrown into one of the three angles of a single well-conditioned or nearly equilateral triangle, we shall find it equivalent to about $\frac{1}{20000}$ of an inch,* being about 0.71 seconds of arc.

The percentage of error here developed is so small that it would not practically vitiate the measurement of lands even in the most valuable localities of great cities.

SUPERIORITY OF THE TRIGONOMETRICAL METHOD.—The degree of precision commonly attained in direct measurements of distances by ordinary methods falls very far short of this, and even of that attainable in angle measurements with the ordinary surveyor's theodolite or transit.

It follows that accuracy, even in common surveying, would be promoted by using the method of triangulation for ascertaining ratios, whenever practicable or convenient, in preference to the common methods of traverse surveying. In the survey of a field, or tract of land, for example, triangulation from one or two judiciously selected and carefully measured bases would give the positions of the corners and other objects with greater accuracy and far less labor than the usual routine of perambulating the outside boundaries, which is now taught in treatises on surveying, and generally practiced by surveyors.

ADVANTAGES OF COMBINING SURVEYING WITH GEODESY.—With the facilities afforded by the Coast Survey triangulations, when carried to the tertiary stage, it is not difficult to perceive that a general method of determining the positions of points by co-ordinates might be established, and that while determinations thus made would be far more satisfactory and definite than those obtained by the ordinary methods of surveying, they would involve no more labor. Under such a system, the field work might consist of such a combination of triangulation and traverse surveying as would be found most convenient under the special circumstances.

To carry out such a system, the trigonometrical stations should be located so near together that two or more of them would be available for any subsequent local survey. If not otherwise visible, there should be convenient arrangments for the temporary erection of signals upon stations to indicate their positions, making them visible from adjacent stations.

TRANSFORMATION OF CO-ORDINATES.—While for geographical purposes, the great co-ordinate planes already described are the most suitable for reference, simplicity and convenience would be promoted by transforming this general co-ordinate system into numerous plane rectangular systems in limited local areas. Accordingly, instead of defining the position of a point by giving its terrestrial latitude and longitude, we would give its "latitude and departure," or its co-ordinates, from the zero point or origin of co-ordinates for the containing area.

For this purpose the local areas, into which the earth's surface is sub-divided, should be made sufficiently small to reduce the error which would arise from considering each separate area, or plane surface, to an inconsiderable amount. Six miles square is not far from the average area of townships in the northern, middle, western, and most of the southern States. The bulge of curvature, or the versed sine of half the arc of 6 miles would be about six feet. This would make the proportional difference between the straight and the curved distance, or between the length of the cord and of the arc, much less than the percentage of probable error inseparable from measurements of the utmost attainable precision in actual practice. Township boundaries, therefore, would seem to afford the most convenient divisions between separate co-ordinate systems. If, however, as is the case in a few of the southern States, no smaller subdivisions than counties exist, these are not usually so large that the use of a single co-ordinate system over its areas would involve any important error.

DIRECTION OF AXES.—The most appropriate and convenient directions for the axes would doubtless be found in meridians and perpendiculars to them,

* More exactly 0.000052 inches, for we have radius = 57,3°, nearly, in terms of arc; or making radius = unity, $1° = \frac{1.0}{57.3}$, $1'' = \frac{1}{206265}$ and $\frac{1}{288000} = 0.71''$ nearly. Conversely in terms of distance, if radius = 15 inches, $\frac{15}{288000}$ = 0.000052 inches.

since azimuths reckoned from the meridional axis would then conform very nearly to the true astronomical azimuth. Owing to the convergence of meridians, there would be a small variation from the true azimuth increasing with the distance from the axis, but no practical error need arise from this cause. The true azimuth, if needed, is easily computed by the formula,

$$\tan.\ \tfrac{1}{2}\ C = \sin.\ L \tan.\ \tfrac{1}{2}\ P,$$

in which L is the middle latitude between the origin and the point where the convergence is to be computed, P the difference of longitude between the same points, and C the convergence sought. In passing from one district to another, however, a certain degree of complication arises, and it becomes necessary to take the convergence into account. We may, without practical error, consider any two adjacent districts as lying in a plane produced by developing a conical surface tangent to the earth on the middle parallel of latitude between the origin of the two districts. On this plane, the convergence of the meridional axes of small districts will conform to the above formula, and the small angle of convergence measures the change of direction between the co-ordinate systems of the two districts.

Passing from one District to Another.—In the passage from one district to another, four different cases of transformation of co-ordinates arise, namely:

1st. When the origin of the new system is east and north of the origin of the old system.

It will, of course, be necessary while assigning positive values to latitudes and departure in one direction to give negative values in the opposite direction; thus if north and east are to be reckoned as positive, south and west must be reckoned as negative.

In this first case, the old co-ordinates of the new origin are, accordingly, positive while the new co-ordinates of the old origin are negative. Or, if we call the old co-ordinates a and b, and the new ones a' and b', a and b are here positive and a' and b' negative.

2d. When the new origin is west and north of the old, or a negative, b positive, a' positive and b' negative.

3d. When the new origin is west and south of the old, or a and b negative, and a' and b' positive.

4th. When the new origin is east and south of the old, or a positive, b negative, a' negative and b' positive.

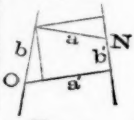
For ascertaining the new co-ordinates of the old origin, the equations—

$$-a' = a \cos.\ \phi + b \sin.\ \phi,$$
$$-b' = b \cos.\ \phi - a \sin.\ \phi,$$

would prove correct in all these cases, provided proper positive and negative values were given to the different terms in the equations. It is necessary to remember, however, that ϕ, the angle of change in direction, between the old and new axes, is positive according to trigonometrical usage when reckoned from zero around towards the left, and negative in the opposite direction, while according to the geodetic method of estimating azimuths, positive angles are reckoned around to the right.

To avoid the confusion which might arise from these different methods of estimating angles, or from assigning a negative value to ϕ, equations are given below for each of the four cases. Their correctness will be apparent on simple inspection of the accompanying figures, in which O and N are the old and new origins respectively:

Fig. 1. Case First.

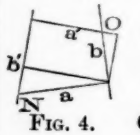


$$a' = a \cos.\ \phi + b \sin.\ \phi,$$
$$b' = b \cos.\ \phi - a \sin.\ \phi.$$

Fig. 2. Case Second.

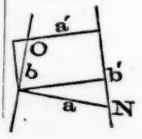
$$a' = a \cos.\ \phi + b \sin.\ \phi,$$
$$b' = b \cos.\ \phi - a \sin.\ \phi.$$

Fig. 3. Case Third.



$$a' = a \cos.\ \phi - b \sin.\ \phi,$$
$$b' = b \cos.\ \phi + a \sin.\ \phi.$$

Fig. 4. Case Fourth.



$$a' = a \cos.\ \phi - b \sin.\ \phi,$$
$$b' = b \cos.\ \phi + a \sin.\ \phi,$$

In these equations the negative sign is omitted before a' and b', but we must remember that they are always estimated in directions opposite to those of a and b.

Since ϕ is a very small angle, *cos.* ϕ approximates closely to unity, and it appears by these equations that a' is greater than a, and b' is less than b when the new origin is farther *north* than the old, while a' is less than a, and b' greater than b when the new origin is farther *south* than the old.

Having found the new ordinates of the old origin, that is of O referred to N, the ordinates of any point referred to the new origin may be computed from its old ordinates by the equations:

$$x = x' \cos.\ \phi - y' \sin.\ \phi + a$$
$$y = x' \sin.\ \phi + y' \cos.\ \phi + b;$$

in which x = departure of any point an referred to N,

x' = departure of the same point as referred to O,

y = difference in latitude of any point as referred to N,

y' = difference in latitude of the same point as referred to O,

a = departure of the origin of O as referred to N,

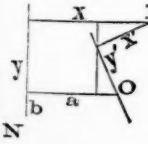
b = difference in latitude of the origin of O as referred to N,

ϕ = the angle of convergence of the meridional axes of N and O.

These are the formulæ for passing from one system of rectangular co-ordinates to another in the same plane.

In these equations, we may consider north and east to be the positive directions, as before, the opposite directions being negative. Also *sin.* ϕ is positive when the change of axial direction is towards the left, and negative when in the opposite direction; that is, positive when N is farther east than O, and negative when farther west. *Cos.* ϕ will always be positive, since ϕ is always either in the first or fourth quadrants.

Fig. 5.



In Fig. 5, the point P, of which x and y are the new co-ordinates, is east and north of the axis passing through O, making x and y greater than a and b respectively, and all the terms of both equations have, accordingly, positive values. But if x, x' or a be estimated towards the west, its sign must be reversed when particular values are substituted in the equations; likewise, if y, y' or b, have a southern direction, its sign must be reversed.

Co-ordinates in a single District.—It is hardly necessary to say that when the azimuth and distance are given from a point whose co-ordinates are known to any other point, the co-ordinates of the latter are found by multiplying the given distance by the sine and co-sine of the azimuth, the first product giving the departure and the other the difference of latitude.

Verifications.—Before finally establishing the co-ordinates of a survey its accuracy should be tested in the most rigid manner, both as regards the instrumental observations and the computations. The computations are easily verified by working to the same point from different directions. Some methods of verifying the field work are indicated in the accompanying plates and explanations. Others will suggest themselves to the surveyor under different circumstances.

Illustrations of Notation.—Plates I and II, exhibit the sort of notation which may be employed under the system proposed. Instead of magnetic bearings or angles written between lines, azimuths are given, which are estimated around to the right from zero at due north to 360° or due north again. From these azimuths, angles around to the right are easily found by subtracting the first azimuth from the second, adding 360° to the latter if zero comes between.

The co-ordinates are here given in feet, but metres, chains or any other standard units may be used in the same way. The letters N. E. S. W. indicate the directions for the origin, north, east, south or west. Points upon the division lines between two adjacent districts have their co-ordinates give in both. For railroad surveys, it would be found convenient in plotting to have the co-ordinates of tangent points and centers of curves given, even though the latter should not appear upon the plan. Other convenient details of notation will suggest themselves to engineers, and indeed the plates are only intended to present to

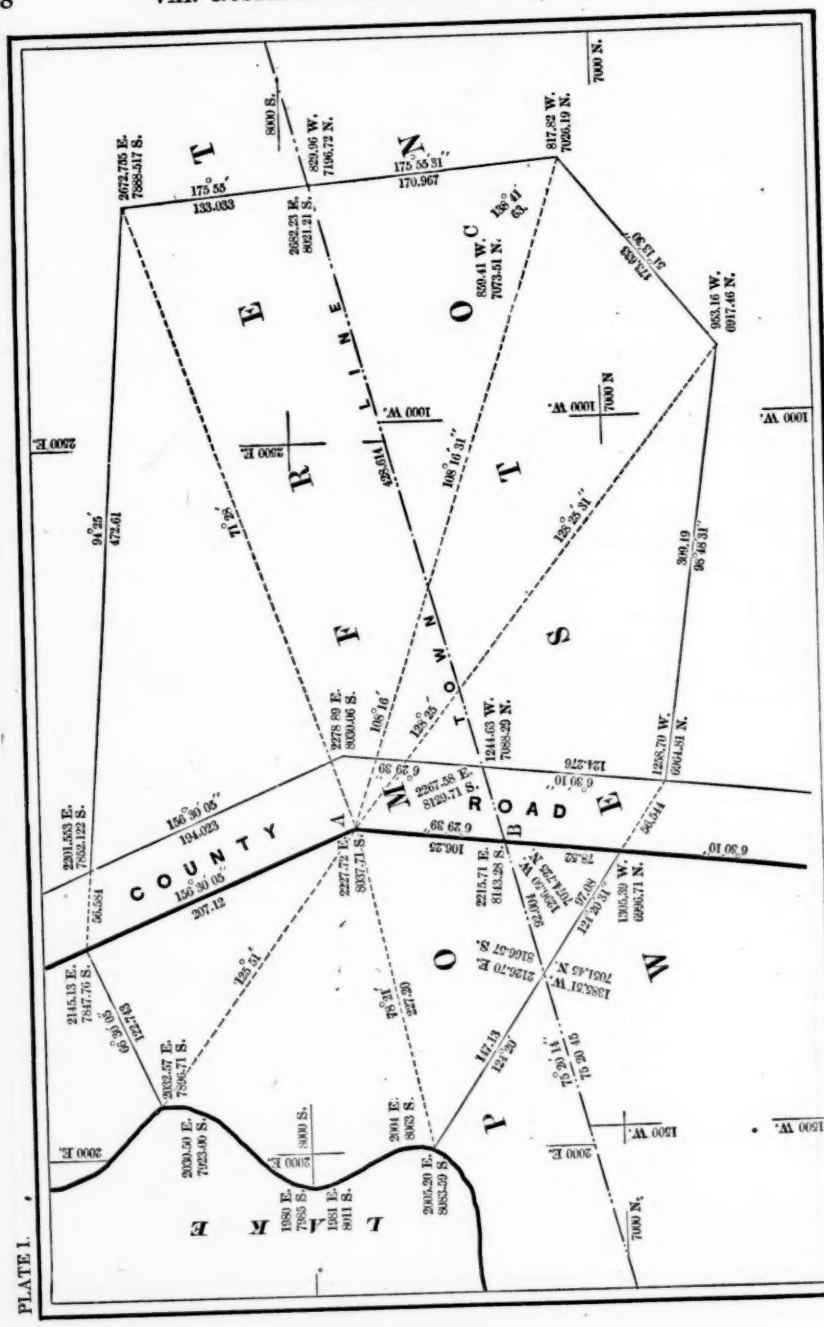
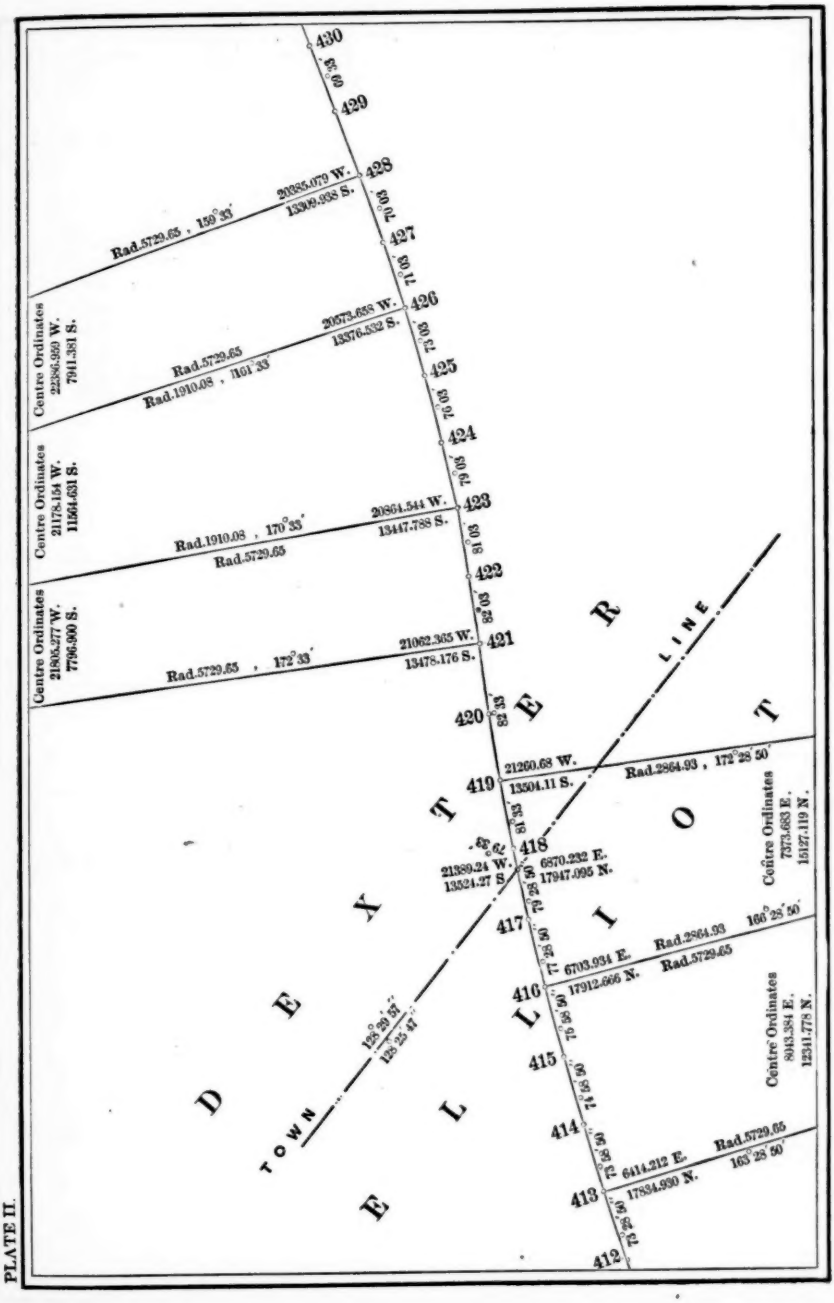

T E N T H L I N E
T O W N S H I P
M A D I S O N
R O A D
C O U N T Y
L A K E
7000 N.
1000 W.
2000 E.
8000 S.
175° 55'
133.033
175° 55' 31"
170.967
2672.735 E.
7988.517 S.
2682.23 E.
8021.21 S.
829.96 W.
7196.72 N.
817.52 W.
7026.19 N.
859.41 W.
7073.51 N.
963.16 W.
6917.46 N.
94° 25'
472.61
T1° 25'
108° 16' 31"
429.614
126° 25' 31"
309.19
98° 16' 31"
2278.89 E.
8030.06 S.
1244.63 W.
7088.29 N.
1258.70 W.
6904.81 N.
124.276
6° 30' 10"
106.25
2267.58 E.
8129.71 S.
2215.71 E.
8143.28 S.
1305.39 W.
6996.71 N.
72.52
2291.553 E.
7852.122 S.
156° 30' 05"
194.023
56.594
207.12
2227.72 E.
8037.71 S.
2145.13 E.
7847.76 S.
2032.57 E.
7986.71 S.
125° 31'
122.19
75° 31'
227.20
2196.70 E.
8166.57 S.
1382.21 W.
7061.45 N.
147.13
121.30
75° 00' 11"
2030.50 E.
7923.00 S.
1980 E.
7965 S.
1951 E.
8011 S.
2001 E.
8063 S.
2005.20 E.
8083.59 S.

PLATE I.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Rad.5729.65 , 159° 33'
20385.079 W.
13309.338 S.
Centre Ordinates
2256.369 W.
7941.361 S.
Rad.5729.65
Rad.1910.08 , 161° 33'
20573.688 W.
13376.532 S.
Centre Ordinates
2178.154 W.
11564.631 S.
Rad.1910.08 , 170° 35'
Rad.5729.65
20864.544 W.
13447.788 S.
Centre Ordinates
2180.577 W.
7796.900 S.
Rad.5729.65 , 172° 33'
21062.365 W.
13478.176 S.
21260.68 W.
13504.11 S.
Rad.2864.93 , 172° 28' 50"
Centre Ordinates
7373.683 E.
15127.119 N.
21389.24 W.
13524.27 S.
6870.232 E.
17947.095 N.
6703.934 E.
17912.666 N.
Rad.2864.93
166° 28' 50"
Rad.5729.65
Centre Ordinates
8043.384 E.
12341.778 N.
6414.212 E.
17834.930 N.
Rad.5729.65
165° 28' 50"
DEXTER LINE
DELLIOTT
TOWN
125° 29' 57"
125° 25' 47"

PLATE II.

the eye the general features of the method proposed. A great variety of cases will arise in practice, many of them requiring special treatment.

COMPUTATION OF AREAS.—Areas are computed under this system with special facility and certainty, the method being the common one of double latitudes and departures. This method is prescribed by law in the State of Ohio, for calculating areas of farming lands and for testing the accuracy of surveys made with the surveyor's compass.

DESCRIPTIONS FOR CONVEYANCES.—For definite and accurate descriptions of land in conveyances, it does not seem possible to devise a more precise and certain method than that of co-ordinates from geodetically determined reference points of origin. The present loose and indefinite descriptions in conveyances, upon which the tenure of a large part of the real estate of the country now depends, are disgracefully uncertain and frequently lead to excessive expenses of unnecessary litigation, and sometimes to costly errors of misplaced constructions.

CONVENIENCE IN CONSTRUCTING MAPS.—In the construction of maps and plans, co-ordinate determinations will be found especially convenient. After completing the survey of any portion of a district it is easy to place it in its proper position upon the map of the entire district, with the certainty that other portions subsequently surveyed will fit into their proper places, without the perplexity and the distortions frequently accompanying the attempts to unite two or more independent surveys made under the methods in common use.

SUMMARY OF ADVANTAGES.—A few of the advantages which may be expected to follow the general adoption of the co-ordinate method of surveying may be summed up as follows:

First.—The attainment of the highest practicable degree of accuracy, as well in smaller local surveys as in more extended operations. The units of measurement which form the basis of the United States Coast Survey have been most carefully compared with those of the entire civilized world, and with the dimensions of the earth itself, and are verified to a degree of precision beyond which the present attainments of scientific skill have not passed.

Second.—Extreme simplicity of notation with ease and convenience of field work and computation.

Third.—Facility in graphic representation.

Fourth.—Absolute certainty of locations in descriptions for conveyances, and consequent removal of a fruitful cause of litigation and trouble.

ALTITUDES.—No change is proposed in the existing methods of determining the third ordinate or altitude. The most convenient mode of fixing this ordinate pre-supposes that the form of the earth's surfaces, or of that surface which would be presented if the irregular surface of the land were razed to the level of the sea, has been accurately determined by geodetic operations, so that in ordinary surveying we have only to ascertain the heights of our points of survey above this imaginary level. This is done by using the spirit level, by measuring vertical angles and by barometrical observations. The first method admits the greatest degree of precision under ordinary circumstances, and is almost exclusively used for engineering purposes.

Plate I, represents a tract of land lying partly in Pomfret and partly in Weston, two adjacent towns. One side is bounded by a lake, and a road passes through the tract. Several of its corners are visible from the point A. The point B, where the town line crosses the west side of the road, is one of the stations fixed by the preliminary trigonometrical survey. All the co-ordinates upon this plan have been determined by computing the latitudes and departures, directly or indirectly, from this point. The convergence of the axial meridians in the two towns is $31''$, and the co-ordinates of the origin in Weston referred to Pomfret are,

$a=3513.27$ E, and $b=15217.81$ S.

From these, we can compute the ordinates of the Pomfret origin referred to Weston, by the formulæ for Case Fourth:

$$a'=a\cos.\ \phi-b\sin.\ \phi,$$
$$b'=b\cos.\ \phi+a\sin.\ \phi,$$

substituting values

(cos. $31''=1$, ϕ log. sin. $31''=6.1769365$)

$a'=3513.27-15217.81\sin. 31''=3510.983$

$b'=15217.81+3513.27\sin. 31''=15218.338$

Reversing the directions,

$a=3510.983$ *W.* $b=15218.338$ *N.*

Equations for passing from Pomfret into Western. The general formulæ are:

$$x=x' \cos.\ \phi-y' \sin.\ \phi+a.$$
$$y=x' \sin.\ \phi+y \cos.\ \phi+b.$$

In this case $\phi = + 31''$; (in passing eastward from Pomfret to Weston the axes swing around to the *left*, and the angle of change is *positive*, according to trigonometrical usage); $a=3510.983$, and $b=15218.328$. Substituting values, the equations become:

$$x=x'-y \sin.\ 31''-3510.983$$
$$y=x' \sin.\ 31''+y'+15218.338.$$

Equations for passing from Weston to Pomfret.—In this case, $\phi = -31''$ (reckoned to the right), $a=3513.27$ and $b=15217.81$; substituting values:

$$x=x'+y' \sin.\ 31''+3513.27$$
$$y=-x' \sin.\ 31''+y'-15217.81.$$

Double pairs of co-ordinates are given along the town line, and either pair may be computed from the other by using these equations. The accuracy both of the equations and the computations are verified by reversing the method.

Verification of the Survey.—At the point C, where three trigonometrical stations can be seen, azimuths were taken to each and the co-ordinates of C computed by the "three point problem." They are 859.36 W, and 7073.59 N, from the origin of Weston. The azimuth from C, to the south-east corner of the tract is 138° 41′, and the distance 63 feet, giving the co-ordinates of C, 859.41 W., 7073.51 N. The degree of accuracy here indicated would probably be sufficient under ordinary circumstances.

Plate II.—In running railway surveys, every opportunity should be taken to connect with the trigonometrical stations which become accessible near the line, so as to verify its direction and the position of the stakes or stations. The methods of doing this by triangulation and otherwise are simple, and will readily suggest themselves to the engineer. This plate illustrates the passage of a railroad survey across a town line, passing from one system of ordinates to another.

The ordinates of the origin in Dexter referred to Elliot, are:

28243.13 E. 31497.21 N.

and the convergence is 4′ 10″.

By the equations given for Case First we find the ordinates of Elliot referred to Dexter to be

(cos. 4′ 10″=1, log. sin. 4′ 10″ =7.0819376.)

$a=28243.13+31497.21 \sin.\ 4'\ 10''$ $=28281.134$

$b=31497.21-28243.13 \sin.\ 4'\ 10''$ $=31463.103$

or reversing directions, 28281.134 W., and 31463.045 S.

Equations for passing from Dexter to Elliot.—The general formulæ are

$$x=x' \cos.\ \phi-y \sin.\ \phi+a.$$
$$y=x' \sin.\ \phi+y' \cos.\ \phi+b.$$

In this case $\phi=4'\ 10''$, $a=-28281.134$ and $b=-31463.045$; and the equations become:

$$x=x'-y'(-\sin.\ 4'\ 10'')-28281.134$$
or $x=x'+y' \sin.\ 4'\ 10''-28281.134$,
and $y=-x' \sin.\ 4'\ 10''+y'-31463.045$.

Equations for passing from Elliot to Dexter.—Here $\phi=4'\ 10''$, $a=28243.13$, $\phi=31497.21$, and the equations become:

$$x=x'-y \sin.\ 4'\ 10''+28243.13$$
$$y=x' \sin.\ 4'\ 10''+y+31497.21.$$

THE "FUTURE" OF STEEL.*

From "Iron."

Until not so very long ago all construction, whether of houses, bridges or ships, depended in point of fact upon the use of stone, brick, or timber, metal of any kind was but little used, except, perhaps, lead, which was to some small extent utilised for roofing, or if they took it for sheathing. In 1758 copper was in use for sheathing, and afterwards was applied in that way throughout the British Navy, but iron was not, either in the way

* A Lecture before the Royal Institution.

of construction or applied to house building, except merely so far as door fastenings and so on, although for more than 500 years it had been used for the mansions of the dead in Sussex, where, to use an eloquent but inaccurate expression, "cast-iron tombstones" were to be found. In 1779 there was the great Coalbrookdale Bridge, and in 1819 Southwark Bridge, which still adorned London. Wrought iron was first used for boats in Staffordshire, and in 1821 a boat was built at Horsley Ironworks, and steamed over to France—this was another instance that substantive inventions were very often made by persons and in localities where they would not naturally be looked for, showing that they were the product of powerful minds unfettered by the traditions of any industry. But by 1830, iron steam-ship building had gone to its appropriate position in a large seaport. In 1830 Messrs. Laird had built their first barge, and from that time there followed a number of vessels —parents of the large fleet which has been built at these works—thus from the beginnings at the Horsley Ironworks and Birkenhead sprung the enormous fleet of wrought-iron ships which are now to be found throughout the world, and, whatever the ownership may be, are generally the product of British naval architectural industry.

But while cast iron and wrought iron were thus used in structures of one kind or another, steel was not so applied: it was a luxury, and cost a shilling a pound or more. It was used for swords, needles, knives, and so on, and for the purposes to which it was put people did not mind paying for it, because they could afford to pay for a certainty—in fact it became a proverb, "As true as steel." He had already said that it would be necessary for him to wander away from the title of his lecture, and now he had to ask his hearers to bear with him while he said something of that metal which made up more than 99 per cent. of the steel which was used—the metal called iron.

Iron, he might say, in its compounds, existed in three forms: cast iron, wrought iron and steel. Cast iron was divisible into three classes, ordinary, chilled, and hardened qualities; there were the ordinary and hardened wrought irons, and steel could be made of such fineness as to have the brightness of glass. Of cast irons, pig-iron was made by a mixture of the ore of iron with some kind of flux, and placing the whole in a blast-furnace—there were blast-furnaces in the Cleveland district which had attained the enormous dimensions of 100 feet in height by 30 feet in diameter, at the part above the boshes, and contained over 40,000 cubic feet of space. Through this enormous tower of material a powerful blowing engine sent a blast which, in the best furnaces and with the aid of the hot-blast stove, reached the temperature of 1250° Fah. Having described the process of smelting inside the furnace, and the great heat attainable by the aid of Dr. Siemens' regenerative appliances, the lecturer showed how the ore became deoxidised, and, by the aid of carbon, ran down to the hearth, when tapping ensued, and the hot metal ran into the pig-bed and became pig-iron. He then exhibited a sample of what is technically known as "sponge of iron." Pig-irons were arranged from number one to number eight, number one containing the largest amount of carbon, the quantity decreasing till the highest number was reached, this being white iron used for malleable purposes. Carbon, in the lower qualities of the iron, was very perceptible, and became less so in the higher numbers, but chemists would tell them that if they wanted total combination they must go to chilled iron. He exhibited a Palliser chilled shot split in two, which showed the extent to which the chilling process had penetrated, and also a bar of iron chilled or hardened at one end and not at the other; the explanation of the process was that the molten metal was suddenly congealed in the metallic moulds, where it was formed, the process taking place so suddenly that the carbon had not time to separate. Malleable iron was made in boxes which could be kept heated for several hours, and where decarbonisation took place producing a metal which was not so brittle as the ordinary cast iron. Some of them could remember that in the exhibition of 1862 there was exhibited a pair of Norwich gates, shown by Messrs. Barnard, Bishop and Company as a sample of art work, and afterwards presented to the Prince of Wales and are now at Sandringham. This iron

could be made with the same ease as other castings; it was rendered malleable, and thus they began with the cheapness of the casting process and ended with the gracefulness of form. With regard to wrought iron, this was made, as they all knew, by taking pig-iron of the higher numbers, 5, 6, 7 or 8, and putting it into a puddling furnace and then stirring about with a rabble or rake. The sides of the furnace were lined with oxide of iron which, acting on the carbon of the pig, formed carbonic oxide of iron, which rose on the top of the mass and looked like little blue flames. This went on till the iron was decarbonised, and then, being made into balls of about one cwt. each, they were taken to the shingling hammer, which was supposed to expel the oxide from between the particles before the mass was rolled into puddled bar. He was afraid the workmen could not stand such an employment for many years, and attempts had been made for a long time past to get rid of that great labor and to apply mechanical motion to the working of the rabble. These efforts had succeeded and the process was in operation, but not largely. But other attempts had been projected, which appeared to be very successful, to change the form of the furnace altogether, and by making that part circular where the iron lay, and so as to operate like a barrel churn, dispense with either mechanical or hand rabbling. Dr. Siemens' rotary puddling furnace was an instance of the latter method, and after describing it the lecturer said the combination after puddling represented carbon, phosphorus, sulphur, manganese and silicon. This wrought iron, with its ½ to 2-10ths of one per cent. of carbon, was what was relied on for kedges, eighty-ton guns, and the like, for years, and very properly so when a better could not be got. It possessed toughness, and, to a certain extent, cheapness, and could be worked from almost any temperature, and would weld, and was altogether a very desirable material. But he had just a word to say as to welding—unless it was of the very best, and they did not know whether it was or not, it was, after all, a very treacherous thing. He had some old wrought iron rails on the table, and in outward appearance they had a dangerous proximity to Landore steel, but they were broken in pieces. The man who made them undoubtedly thought they were welded properly, but, as a matter of fact, they were merely bundles of fagots. He had also a piece of boiler-plate from a first-rate maker's that had been placed in a boiler which went to work, then a blister appeared because that particnlar plate had not been properly welded, and this caused the boiler to be unsafe. These were some of the difficulties which had to be met with in dealing with wrought iron.

When steel was first made it was manufactured in a sort of petty way. One of the diagrams showed the process by cementation which made blistered steel, and until 1750 a welding process produced an objectionable material called shear steel. After this, Huntsman went to work, and finding the material was fusible, the metal was fused in pots or crucibles holding half cwt. each, and then poured into pots, and until 1851 this was the steel at 1s. per lb. The quality containing from 1.0 to 1.2 per cent. of carbon was the hardest steel, used for files, etc., and the mildest form, containing from ½ to 6-10ths. In 1851 Krupp startled all the makers by making one ingot of 4,500 lb. weight, for the greatest thing that any Sheffield maker had then done was to put two pots together. But Krupp took a lot of pots and, having drilled his men, simply poured the whole in one mass. But in 1862 there was exhibited a mass of twenty tons weight, and this was found perfectly sound from end to end when cut open.

A French chemist, Chenot, next made an ingenious attempt to obtain steel direct from the ore. After the ore became deoxidised by his process, it was ground into powder and then mixed with carbonic material. The whole powder was put into a machine which pressed it, and then it was fused, the earthy matter left rising to the top. The remainder was then pressed into a mould, and the best steel he (the lecturer) had ever seen was the result. This was a good original idea, but it was not cheap, and nothing came of it in England or, he believed, elsewhere. In 1850 Riepe attempted to make steel in a cheaper way. In making iron the material inside the

furnace had to come down to a condition lower than that required to produce steel, so it was argued that the right point might be hit upon during the process of making iron. But Riepe did not succeed in hitting the right moment, and he was sorry to say that as a consequence of these experiments a good deal of harm was done. The product was not successful, and it could be understood how and why, when it was recollected that the steel manufacturer was careful even to half-tenths of carbon being present in his steel. The steel thus produced, however, was thought well of, and it was largely used at the beginning; girders were made with it, and the Board of Trade officials inspected them—and they said they were not so good as wrought iron. So steel got a bad name, which it has not lost even to this day. After Riepe came Bessemer, who practically converted steel from being a luxury into that which will be used eventually instead of wrought iron everywhere. Describing the Bessemer converter and its functions,* the lecturer went on to say that steel thus made did not, however, regain the character lost under Riepe's process. The exact effects of every preparation could not be ascertained, and some tests showed that the metal would bear 20, 30 or 40 tons of pressure. It was said at last that the metal could not be depended upon, and it would be a poor satisfaction to some injured passenger if the manufacturer of a rail said the accident should not have happened according to his averages; the passenger would at once reply that he cared nothing for the averages but looked to the minimum. He would quote the following words uttered by Mr. Barnaby in 1875: "I should be very doubtful of a ship built of it unless I could see every plate worked." Now, he thought Mr. Barnaby judged very fairly considering the condition of things at that time, but he did not remember that many able men were then turning their attention to steel manufacture. Among those able men was one of the managers of the Royal Institution, Dr. Siemens, who had probably done more to restore the good character of steel than any other man in England. He has described by the aid of diagrams the principle of the Siemens regenerative furnace. By this process about thirteen charges of ten or twelve tons each could be worked in one of these furnaces per week with a minimum of labor, consisting of shoveling in the material every three hours, while there was the invaluable opportunity of taking samples of the mass, thus reducing its ultimate character to an absolute certainty. Thus, while it might be said that by the Bessemer process it was a difficult thing to make good steel, by the Siemens process, it really required a good deal of trouble to make bad steel. In this way the character of the metal having been made a certainty, and manufacturers having improved their steel, Mr. Barnaby's remarks last year were to the effect that it was a "splendid material, such as shipbuilders might use with confidence. Steel makers," added Mr. Barnaby, "might not have taken his remarks of the previous year kindly, but he would ask them whether there was not a good deal better class of metal now than they had then?" Steel was now used by the Admiralty with confidence. They demanded that a sample eight inches long should before ultimate fracture extend 20 per cent. of its own length, and after heating and quenching in water should bear bending round to a radius of $\frac{3}{4}$ of its own thickness. Of 14,000 samples not one had failed, and it was seen that steel would do more than that. (Here, as an illustration, a steel bar was torn asunder in a small testing machine.)

He thought he had shown them that steel had now recovered its character, and that they could again say, with all confidence, "As true as steel." Let this once be the conviction in the minds of constructors and they would use it. They were prompted to do so, he was glad to say; and owing to the assistance of Sir John Hawkshaw, and the committee of the British Association at Bradford, the Board of Trade has been so plied with arguments, that he believed they were on the point of yielding, so that steel would be recognised at its proper value. Holland had recognised it, and it was a pity that the country which was the home of the metal should be behind in this respect. He believed steel would supersede iron in almost everything; for instance, they could not go on making bridges with large spans by merely adding to the material in order

to add to their strength, for the weight would tell against the structure, and therefore when such were required, the only question was whether they would be allowed to use steel. There was no doubt, he thought, the steel and wrought iron overlapped each other, but steel was really to be the metal of the future, for eventually iron would only be used in two forms—cast iron, for purposes where great weight was required, and wrought iron for the purposes of ornamentation, and then they would have steel. Riepe, by a single moulding process, had enabled them to make spectacular steel, which could be run and moulded with the ease of ordinary castings, and in his (the lecturer's) opinion, the future of steel was that it would occupy every province hitherto occupied by wrought iron, except the welding and forge work of ordinary blacksmiths. Mr. Bramwell, who stated that his preliminary observations had only left him three minutes to devote to the title of his lecture, then concluded his remarks.

PROCESSES AND PRODUCTS OF IRON MANUFACTURE.*

By Dr. C. W. SIEMENS, F. R. S.

From "The Engineer."

Having thus dwelt—too long I fear, for your patience—upon the subject of fuel, I now approach the question as to the processes by which we can best accomplish our purpose of converting the crude iron ore into such materials as leave our smelting works and forges. The subject of blast furnace economy has already been so fully discussed by you, during the term of office of your past President, Mr. I. Lowthian Bell, M.P., F.R.S., who has done so much himself to throw light upon the complicated chemical reactions which occur in the blast furnace, that I may be permitted, on the present occasion, to pass over this question, and to call your attention more particularly to those processes by which iron is made to attain its highest qualities, both as regards power of resistance and ductility. Iron and steel were known to the ancients, and are referred to in their works, but we have no account of the processes employed in their manufacture until, comparatively speaking, recent times. Aristotle describes steel as purified iron, and says that it is obtained by re-melting iron several times, and treating it with various fluxes. We are hence led to suppose that in Aristotle's time steel was made by careful selection and treatment of steely iron, which latter was produced by something analogous to the Catalan process. A method referred to by ancient authors is to bury iron in damp ground for some time, and then to heat and hammer it. Another process, described first in Biringuccio's "Pyrotechnology," one of the earliest works on metallurgy, and later in Agricola's "De Re Metallica," both published in the sixteenth century, is to retain malleable iron for some hours in a bath of fused cast iron, when it becomes converted into steel. Reaumur, in 1722, produced steel by melting three parts of cast iron with one part of wrought iron (probably in a small crucible) in a common forge, but he failed to produce steel in this manner upon a working scale. A similar method of producing steel to that proposed by Reaumur, has been employed in India for ages, the celebrated Wootz steel being the result of partial or entire fusion of steely iron and carbonaceous matter, in small crucibles arranged in a primitive air furnace, followed by a lengthy exposure of the ingots to heated air in order to effect a partial decarburisation. In 1750, Hasenfratz refers in his "Siderotechnic," to three processes for producing steel—melting broken fragments of steel with suitable fluxes, fusing malleable iron with carbonaceous matter, and so treating cast iron, probably with oxides, as to obtain cast steel directly from it. The credit of producing cast steel upon a working scale is

*Abstract of Inaugural address of Dr. Siemens before the Iron and Steel Institute.

due to Huntsman, who was the first to accomplish its entire fusion in crucibles, placed amongst the coke of an air furnace, pouring the fluid metal produced into metallic moulds. This process is still carried on largely at Sheffield for the production of steels of special qualities, such as tool steel, tire steel, castings and forgings, and a ton of cast steel in ingot is produced with the expediture of from 2½ to 3 tons of Durham coke, according to the degree of mildness of the metal produced. At Pittsburgh, where pot-melting is employed on a considerable scale, plumbago pots are invariably used of nearly double the capacity of the clay pots used at Sheffield; eighteen or twenty-four of these pots, each containing about a hundred weight of metal, are placed in a furnace, each pot lasting twenty-four hours, and yielding five charges during that interval. The fuel consumed amounts to one ton of small slag per ton of steel melted, and is delivered to the works at the surprisingly low price of thirty cents per ton. With these important advantages in his favor, the American steel melter should be able, one would think, to meet without protection his Sheffield competitor in the open market. As regards Bessemer steel, great advances have been made in recent times in cheapening production. At Creusot, and other Continental works, a system of direct working, or of transfering the pig metal in the molten condition from the blast furnace to the Bessemer converter has been introduced, and the same method has been recently adopted at several of the leading English works. By this method of working the fuel usually employed in remelting the pig metal in the cupola—say 2½ cwt. per ton—is clearly saved; and other advantages are realized, but on the other hand, the Bessemer converter is made dependent upon the working of the blast furnace both as regards time and the quality of the resulting metal. At Barrow, and other large works, where a number of blast furnaces supply a number of Bessemer converters, and pig metal for the open market in addition, this mode of working appears to be practically free from the objection above stated, and a hot ladle, with its engine, may be kept steadily at work transferring the pig metal from one blast furnace or another, to the converters. But it still remains to be seen whether any practical advantage can be realized by this method of working at smaller works, where a change in the working of the blast furnace from Bessemer to forge pigs, would cause a serious interruption in the working of Bessemer plant.

In America the effort of the iron-master has been directed—chiefly under the guidance of Mr. A. L. Holley—towards a saving of labor, by increasing to an almost incredible extent the number of blows per diem from each converter. Thus I was informed that at the North Chicago Steel Works as many as seventy-three blows had been obtained in one pit in twenty-four hours, although I have reason to doubt whether this rate of working could be maintained for any length of time. The Americans have not adopted, so far as I could ascertain, the direct process of working, but are content to remelt their pig metal in large cupolas in immediate proximity to the converters; the capacity of the converters has latterly been much increased, and the degree of heat engendered by a blast of increased power, has been augmented to such an extent that a considerable amount of scrap metal can be remelted within the fluid bath before discharging the same into the ingot moulds. Whilst the Bessemer process has been making rapid strides, a rival process has gradually grown up by its side, which I cannot pass over without remark. I allude to the open hearth steel process, with which my name and the joint names of Siemens and Martin are associated. The conception of this process is really as old as that of cast steel itself. The ancient Indian steel, the Wootz, was the result of a fusion of a mixture of malleable and cast iron. Reaumur, as already stated, proposed to melt wrought iron and pig metal together, for the production of steel, as early as 1722; and G. B. Heath—to whom we owe the important discovery that by the addition of manganese to cast steel its malleability is greatly increased—endeavoured to realize the conception of producing steel in large masses upon the open hearth of the furnace in the year 1839, and he again has been followed in these endeavors by Gentle Brown, Richards, and others in the same direction. When, in

1856, I first seriously gave my attention, in conjunction with my brother, Frederick Siemens, to the construction of a regenerative gas furnace, I perceived that this furnace would be admirably adapted to the production of steel upon the open hearth, and I remember proposing it for such a purpose to Mr. Abraham Darby, of Ebbw Vale, in 1861. Ever since that time I have been engaged in the realization of this idea, which has been retarded, however, by those untoward circumstances which ever intervene between a mere conception and its practical realization. Although two of my earlier licencees, Mr. Chas. Attwood, of Tow Law, and the Fourehambault Company, in France, with whom was my late esteemed friend, Mons. Lechatelier, Inspecteur-General des Mines, succeeded, in 1865 and 1866, in producing steel upon the upon hearth, they did not persevere sufficiently to attain commercial results; and it was not until after I had established experimental steel works at Birmingham that I was enabled to combat in detail the various difficulties, which at one time looked well-nigh insuperable. Whilst thus engaged, Messrs. Pierre and Emile Martin, of Cereuil—who had obtained licenses for furnaces to melt steel both in pots and on the open hearth—succeeded, after a short period of experimenting, in introducing into the market open-hearth steel of excellent quality.

Whilst Messrs. Martin thus gave their attention to the production of steel by the dissolution of wrought iron and steel scrap in a bath of pig metal, my own efforts were more especially directed to the production of steel by the use of pig metal and iron ores, either in the raw state or in a more or less reduced condition, which latter process is the one mostly employed in this country. One of the advantages that may be claimed for the open-hearth system consists in its not being dependent upon a limited time for its results. The heat of the furnace is such that the fluid bath of metal, after being reduced to the lowest point of carbonisation, may be maintained in that condition for any reasonable length of time, during which samples may be taken and tested, and such additions, either of pig metal, of wrought scrap, spongy metal, or ore, may be made to it as to adjust it to the desired quality. Spiegeleisen, or ferro-manganese, is then added in the solid condition, in the requisite proportion, and the result is a bath of metal, the precise chemical condition of which is known, and which has the advantage, if properly managed, of being what is technically called "dead melted," which circumstance renders it applicable for certain purposes for which pot steel has hitherto been mostly employed. The purpose for which the open-hearth process is more especially applicable has reference to the conversion of scrap steel, and iron of every description into steel or ingot metal, and it is now used, indeed, to a large extent, for the conversion into steel of old iron rails. The wearing qualities of these converted rails have been under test since 1867, when the Great Western Railway Company had some old Dowlais iron rails converted into steel at my experimental steel works at Birmingham, which was rolled into rails by Sir John Brown and Co., and which have been down ever since that time at Paddington, subjected to great wear and tear. The manufacture of steel, both by the Bessemer and the open-hearth process, is much facilitated by the use of ferro-manganese. This material was introduced into the market in 1868, by Mr. Henderson, of Glasgow. It was produced successfully by charging carbonate or oxide of manganese and manganiferous iron ore intimately mixed with carbonaceous matter upon the open-hearth of a Siemens furnace with a carbonaceous lining; but the demand for this material was not sufficient to render the manufacture profitable at that time, and it was not until the year 1875 that it was re-introduced into the market by the Terrenoire Company. Manganese, when added in a proportion of .5 per cent., or more, to steel or ingot metal containing only from .15 to .20 per cent. of carbon, has the effect of removing red-shortness, and of making it extremely malleable both in the heated and cold conditions.

In using spiegeleisen containing only from 10 to 15 per cent. of metallic manganese, it is impossible to supply the amount necessary to produce this malleability without adding, at the same time, such a percentage of carbon as would produce a hard metal. The use of ferro-manganese enables us to overcome this difficulty, and greatly facilitates the

production of a metal so malleable and with so little carbon, as to remain practically unaffected in its temper when plunged red-hot into water. Another result produced by the use of manganese without carbon, upon mild steel or ingot metal, is to neutralize the objectionable effect of phosphorus, so long as the latter does not exceed the limit of .25 per cent. This metal, in which phosphorus may be said to take the place of carbon, presents a large specular fracture, and is, contrary to what might have been expected, extremely ductile when cold. Iron when in the fluid condition can be alloyed with other metals, and some of the compounds thus formed are known to possess very remarkable properties. Thus, iron combined with 3 per cent. of tungsten and .8 per cent. of carbon, yields a metal which can be worked like ordinary steel, but which, when hardened, retains magnetism to a very remarkable degree. A further addition of tungsten produces an exceedingly hard metal (introduced into the market by Mr. Mushet) which cannot be forged, but which when cast into bars, and ground so as to form a shap edge, produces cutting tools capable of great endurance. An admixture of chromium has for many years past been known to produce steel of great hardness and strength, but it is only quite recently that it has been brought into practical use in America by Mr. Julius Baur, and has been taken up in this country by Sir John Brown, and Co., of Sheffield, who claim for it very remarkable properties as regards strength, malleability, and fredom from corrosion. The formation of compounds such as these is a matter of great interest in connection with the future development of the applications of steel, and is one of those subjects which I venture to suggest might be much advanced by an organized research, under the auspices of a committee of the Iron and Steel Institute. The value of the material known as mild steel or ingot metal consists in its extreme ductility under all possible conditions. Its ultimate strength is much inferior to that of ordinary steel, and rarely exceeds 28 tons per square inch; its limit of elasticity is reached at 15 tons per square inch, whilst the limit of elasticity of a harder steel may reach from 25 to 30 tons per square inch, and that of hard drawn steel wire from 45 to 50 tons.

But in estimating the relative value of these different materials by the amount of work that has to be expended in causing rupture, it will be found that the mild steel has the advantage over its competitors. When subjected to blows or sudden strains, such as are produced by the explosion of gun-cotton or dynamite, extra mild steel differs in its behaviour from that of BB iron and ordinary steel, by yielding to an extraordinary extent without fracturing, and it is in consequence of this non-liability to rupture that it may be loaded to a point much nearer to its limit of elasticity than would be safe with any other material. Attention has been recently directed in various quarters to remedy a defect appertaining to steel, that of piping and showing honeycombed appearance in the ingot. It is well known that if such steel is hammered and rolled, the open spaces contained in it are elongated, and seemingly closed up, but in reality continue to form severances within the metallic mass, to the prejudice of the uniform strength of the finished forging. In casting steel containing more than .5 per cent. of carbon, the defect of honeycombing can easily be avoided if care is taken to have the metal "dead melted" before casting it into the mould; and that of piping in continuing the inflow of fluid metal for a sufficient length of time while it is setting. But in dealing with mild steel containing only say .2 per cent. of carbon, the difficulty of making a sound casting is greatly increased. Much may be done, however, by careful manipulation of the fluid metal, and by the judicious addition to it of manganese or other oxidisable metal, such as silicon or lead, by which occluded oxygen is removed. Sir Joseph Whitworth, who, as you well know, has given much attention to this subject, has overcome the evil mechanically by subjecting the steel, while setting in the mould, to great hydraulic compression. He has thus succeeded in producing, in large masses, mild steel of extremely uniform strength, and the only doubt which could possibly be raised against the advisability of producing fluid steel for ordinary applications by this method is on the question of expense. The subject of producing

sound steel castings is one which we shall have an opportunity to discuss in reference to a paper which will be presented by M. Gautier.

The employment of steel for general engineering purposes dates only from the year 1851, when Krupp, of Essen, astonished the world by his exhibits of a steel ingot weighing 2,500 lbs., and of his first steel gun, and introduced a comparatively mild description of pot steel for steel tires, axles, and crank shafts. For the production of these he constructed his celebrated monster hammer, with a falling weight of 45 tons, which at that time far surpassed in magnitude and power our boldest conception, and is now only being exceeded by a still more powerful hammer in course of erection at the Essen Works. Krupp's steel was, however, not cheap steel, and it is to our past President, Mr. Henry Bessemer, that we are indebted for the production of steel at such a reduced cost as to make it available for railway bars and structural purposes, in substitution for iron, since which event the applications of this superior material show a most extraordinary rate of increase. Not only do we travel upon steel tires, running over steel rails, but at least one of our leading railway companies, the London and North Western, has, under the able management of Mr. F. W. Webb, constructed as many as 748 locomotive engines, including boiler, frame, and working parts, entirely of that material, excepting only the fire-boxes, which are still made of copper. In France, also, much attention has been given to the introduction of steel for machinery purposes, and there, as well as in the United States, Germany, and Holland, that material is used largely in the construction of bridges and other engineering works. In this country, the application of steel for structural purposes has occupied the attention of some of our leading civil engineers for many years, and Sir John Hawkshaw, when called upon to construct a railway bridge at Hungerford, in 1859, proposed the use of steel in order to lighten the structure. He was prevented, however, from carrying his idea into effect by the rules of the Board of Trade, which provide that any kind of wrought material shall not be weighted either in compression or extension to more than five tons per square inch. Repeated efforts have been made since that time to induce the Board of Trade to adopt a new rule, in which the superior strength of steel should be recognized, and in order to facilitate their action a committee was formed, consisting of Mr. William Henry Barlow, Captain Galton, and others, who carried out—with the pecuniary aid of leading steel manufacturers—a series of valuable experiments, showing the limit of elasticity and ultimate strength of various steels, which results are published separately in the "Experiments on the Mechanical and other Properties of Steel, by a Committee of Civil Engineers." At the instance of Mr. Barlow, the British Association appointed a further committee to promote the object of obtaining for steel its proper recognition, and this has led finally to the appointment under the sanction of the Board of Trade of three gentlemen, viz., Sir John Hawkshaw, F.R.S., and Mr. William Henry Barlow, F.R.S., who were nominated by the Council of the Institution of Civil Engineers, and of Colonel Yolland, F.R.S., of the Board of Trade. These gentlemen have agreed upon a report recommending the use of steel as a building material, subject to a limit of strength greatly in excess of the limit assigned to wrought iron, and it is to be hoped that the Board of Trade, by adopting that report, will remove the serious drawback which has too long stood in the way of the application of steel for structural purposes, and which has rendered the construction of large works, such as the projected bridge over the Firth of Forth, practically impossible. As regards the construction of ships of extra mild steel, the English Admiralty, following the example set by France, has, under the advice of Mr. Barnaby, the Chief Constructor, taken the lead of the commercial navy of the country, and several corvettes have recently been constructed entirely of that material, at the Government yard at Pembroke, and upon the Clyde.

The constructors of merchant shipping have been hitherto restricted by rules laid down by Lloyd's Registry, which makes no distinction between common iron and steel in determining the classification of a vessel. It is to be hoped

that the important engineering and ship-building interests of the country will soon be released from regulations which may have been well adapted to the use of an inferior material such as common iron, but fail entirely to meet the requirements of the present day. In ship-building, the use of a material superior in toughness and in strength produces the double advantage of greater safety to life and property, and of an increase of carrying capacity to the full amount of weight saved in the construction of the ship. It should be borne in mind that this additional weight of merchandise is carried without increasing the working expenses and power required to propel the ship, and may just suffice to strike the balance between working a vessel designed for long voyages at a fair profit or a loss.

In constructing the masts and yards of vessels of the stronger material, the weight saved is a matter of still greater importance, which I am glad to say now engages earnest attention. In the United States, a committee, composed of both military and civil engineers, have been engaged for some time upon the subject of determining experimentally the structural value of iron and steel, with the advantage of substantial support from the United States Government, who, after a previous grant of 75,000 dollars, have, I observe, granted a further sum of 40,000 dollars in aid of the experimental inquiries instituted by the committee. The council of the Iron and Steel Institute are not unmindful of the importance of this subject, and have invited those gentlemen of this and other countries, who have given most attention to the production and application of steel, to aid us in our forthcoming discussion with the results of their experience. In the course of this discussion the distinctive limits between steel and iron will necessarily engage your attention. Considering the extraordinary change of physical condition which iron undergoes when alloyed with small percentages of carbon, manganese, phosphorus, tungsten, chromium, and other substances, and considering, further, that it is never quite free from some admixture, the question of nomenclature is one naturally surrounded with difficulty, but it is becoming one of considerable practical importance when rules are to be laid down regulating the permissible strength of different grades of these materials. Dr. Percy has, in his "Metallurgy of Iron and Steel," defined steel as iron containing a small percentage of carbon, the alloy having the property of taking a temper; and this definition is substantially equivalent to those found in the works of Karsten, Wedding, Gruner, and Tunner. On the other hand, Messrs. Jordan, Greiner, Gautier, Phillipart, Holley, and others define as steel all alloys of iron which have been cast in malleable masses, whilst Sir Joseph Whitworth considers that steel should be defined mechanically by a co-efficient representing the sum of its strength and ductility. With the object of settling this question of nomenclature, an International Committee was appointed at Philadelphia, by the Institution of American Mining Engineers. The committee consisted of the following gentlemen:—Mr. I. Lowthian Bell, M.P.; Dr. Hermann Wedding; Professor Tunner; Professor Akermann; M. Gruner; and Messrs. A. L. Holley and T. Egleston, and they resolved that the following should be recommended:

(1) That all malleable compounds of iron, with its ordinary ingredients, which are aggregated from pasty masses, or from piles, or from any form of iron not in a fluid state, and which will not sensibly harden and temper, and which generally resemble what is called wrought iron, shall be called weld iron (German, *Schweisseisen;* French, *fer soude*). (2) That such compounds when they will from any cause harden and temper, and which resemble what is now called "puddled steel," shall be called weld steel (German, *Schweiss-stahl;* French, *acier soude*). (3) That all compounds of iron, with its ordinary ingredients, which have been cast from a fluid state into malleable masses, and which will not sensibly harden by being quenched in water while at a red heat, shall be called ingot iron (German, *Flusseisen;* French, *fer fondu*). (4) That all such compounds, when they shall from any cause so harden, shall be called ingot steel (German, *Fluss stahl;* French, *acier fondu*)." The nomenclature here proposed is entitled to careful consideration from the eminence for both theoretical

and practical knowledge of the gentlemen composing the committee; but I apprehend that for common use the distinctions desired to be drawn are too manifold. Moreover, the lines of demarkation laid down run through materials very similar, if not identical, in their application, where a distinction in name would be extremely difficult to maintain and awkward to draw. Take, for instance, railway bars from ingot metal, which are usually specified to bear a given dead load without deflecting beyond certain limits, and to resist certain impact without rupture. The materials answering to these requirements contain from .2 to .6 per cent. of carbon, depending in a great measure upon the mode of production, and upon the amount of admixture of phosphorus, sulphur, silicon, and manganese. But inasmuch as the quality of tempering depends chiefly upon carbon, part of the rails delivered under such specification might have been classified as ingot iron, and part as ingot steel. The committee omits to define the degree of hardening which it considers necessary to bring a material within the denomination of ingot steel. It is well known, however, that the temper depends upon the exact temperature to which the metal is heated before being plunged into the refrigerating medium, and also upon the temperature and conductivity of the latter, and that ingot metal with even .2 per cent. of carbon, when plunged hot into cold water, takes a certain amount of temper. The question of the amount of import duties payable in foreign countries upon metal occupying a position near the proposed boundary line, would also lead to considerable inconvenience. Difficulties such as these have hitherto prevented the adoption of any of the proposed nomenclatures, and have decided engineers and manufacturers in the meantime, to include, under the general denominations of cast steel, all compounds consisting chiefly of iron, which have been produced through fusion, and are malleable. Such a general definition does not exclude from the denomination of steel, materials that may not have been produced by fusion, and which may be capable of tempering, such as shear steel, blister steel, and puddled steel, nor does it interfere with distinctions between cast steels produced by different methods, such as pot steel, Bessemer steel, or steel by fusion on the open hearth. The forthcoming discussion will, I hope, lead to some general agreement regarding this question of nomenclature.

While steel is gradually supplanting wrought iron in many of its applications, efforts are being made to maintain for the latter material an independent position, for cheapness and facility of manipulation, by improving the puddling process. Mechanical puddling, like many other important inventions, has taken a long time for its development, and has engaged the attention of many minds, but I will only here mention the names of Tooth, Yates, and Mr. Menelaus, our past president, who have pioneered the road; and of Danks, Spencer, Crampton, and others who have followed more recently in the same direction. It is chiefly owing, however, to the persevering endeavors of Mr. Heath, and of Messrs. Hopkins, Gilkes, and Co., that the mechanical puddling of pig metal has been accomplished with a considerable amount of success. All these efforts have had reference to puddling in a chamber rotating upon a horizontal axis, but numerous attempts have also been made to accomplish mechanical puddling by the introduction into stationary chambers of rabbles moved by mechanical power, and by the use of chambers rotating upon an inclined axis, in connection with which latter the names of Maudslay, Sir John Alleyne, and Pernot should be mentioned. The principal difficulty connected with the rotary puddling furnace consisted in providing a lining of sufficient power to resist the corrosive action produced by siliceous slags, and it is important, therefore, that the pig metal introduced into the rotative puddler should be as free from silica as possible. By charging fluid metal into the furnace, the silica adhering to the pigs in the form of sand is got rid of; but efforts have latterly been made, with satisfactory results, I believe, to subject the pig iron itself to a simple finery process on its way from the blast furnace to the rotative puddler, with a view of removing the silicon chemically combined with the pig. M. Hamoir, of Belgium, has been engaged upon this subject for some years, as you will have

seen from the "Report on the Progress of the Iron and Steel Industries in Foreign Countries" in our "Journal;" while, in this country, Mr. I. Lowthian Bell has called the Bessemer converter into requisition for effecting the desired object. We are informed that not only does the lining of the furnace stand better in using this semi-refined metal, but that the yield per furnace per diem, as well as the quality of the metal obtained, are much improved. It is intended to roll the metal thus produced into railway bars, without any intermediate process of re-heating, and to subject the rails to a process of case-hardening similar to what was practised some years ago by Mr. Dodds, in South Wales. The case-hardened iron rails are expected to rival steel rail in quality, but it remains to be seen whether these wearing properties are not obtained at the cost of brittleness, and whether rails manufactured by this method can compete in price with steel rails. Three years ago, I had the honor of bringing before this Institute a plan of producing wrought iron directly from the ore, in a rotative furnace of special construction, and heated by gas. This process was at that time only carried on upon a small scale at my sample steel works, in Birmingham. It has since been carried out upon a working scale, at Towcester, and in Canada, and although the results hitherto obtained cannot yet be considered entirely satisfactory from a commercial point of view, I see no reason to feel discouraged as regards the ultimate result of this method of treating iron ores. By it, iron of almost entire freedom from sulphur and phosphorus is obtained from ores containing a considerable percentage of these impurities. If steel is to be produced, the raw balls, as they leave the rotary furnace, are either immediately transferred to the bath of the open-hearth furnace, or are previously subjected to the processes of squeezing and hammering for the removal of scoria, which otherwise carries some of the impurities contained in the ore into the metallic bath, and prevents the attainment of steel of a high quality. One of the drawbacks to the use of iron and steel for structural purposes is found in their liability to rust when exposed to air and moisture. The ordinary means of protection against rust consists in covering the exposed surfaces with paint, and if this is renewed from time to time, iron and steel may be indefinitely preserved from corrosive action. Another mode of protection consists in dipping articles of iron and steel while hot into a bath of oil, when some of the oil penetrates to a slight depth into the pores of the metal, while other portions become decomposed, and form a very tenacious resinous coating. For the protection of iron and steel when in the form of thin sheets or wire, galvanising, as is well known, is largely resorted to. The principle of protection in this case depends upon the fact that zinc, although more oxidisable than iron, forms, with oxygen, an oxide of a very permanent nature which continues to adhere closely to the metal, and thus prevents further access of oxygen to the same. This mode of protection presents the further advantage that so long as any metallic zinc remains in contact with the iron in presence of moisture, the latter metal forms with the zinc the negative element of an electrolytic couple, and is thus rendered incapable of combining with oxygen. Galvanising is not applicable in those cases in which structures of iron and steel are put together by the aid of heat, or are brought into contact with sea-water, which would soon dissolve the protecting zinc covering. But even in these cases the metal may be effectually protected against corrosion by attaching to it pieces of zinc, which latter are found to dissolve in lieu of the iron, and must, therefore, be renewed from time to time. Captain Ainslie, of the Admiralty, has lately made a series of valuable experiments, showing the relative tendency towards corrosion of both iron and steel when in contact with sea-water, and of the efficacy of pieces of zinc in preventing this corrosion. These experiments further show that mild steel is—contrary to the results obtained by M. Gautier—more liable to corrosion than wrought iron in its unprotected condition, but that zinc acts most efficaciously in protecting it. Quite recently, another mode of protecting iron and steel plates from corrosion has been suggested by Professor Barff. This consists in exposing the metallic surfaces while heated to redness, to the action of superheated steam, thus producing upon

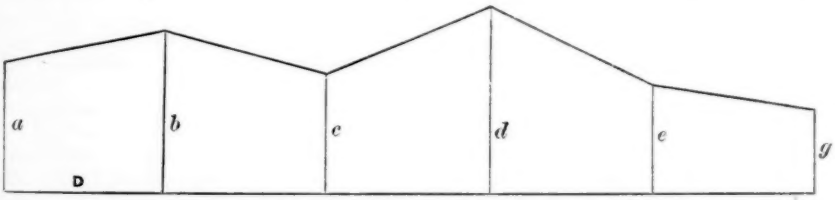
their surface the magnetic oxide of iron, which, unlike common rust, possesses the characteristic of permanency, and adheres closely to the metallic surface below. In this respect it is analogous to zinc oxide adhering to and protecting metallic zinc, with this further advantage in its favor, that the magnetic oxide is practically insoluble in sea water and other weak saline solutions.

CENTER OF GRAVITY OF EARTHWORK.

By J. WOODBRIDGE DAVIS, C. E.

Written for Van Nostrand's Engineering Magazine.

To the engineer, engaged on the construction of a railroad or canal, perhaps no problem is more constantly presented than that of finding the average haul of a piece of excavation. It is shown in all treatises that the average haul is the distance between the center of gravity of the material as found to its center of gravity as deposited. It is, furthermore, shown that to find the center of gravity in either place, we must use the principle of moments, dividing the mass into elementary volumes, multiplying each by its distance from an assumed axis, adding these products and finding what distance multiplied by the whole volume will produce this sum. The accompanying diagram represents a longitudinal section



of that part of a cutting whose material has been transported in one direction. We know the volumes, V_1, V_2, etc., between the equi-distant cross sections, *a* and *b*, *b* and *c*, etc.; and, consequently, we know the volume, V, of the series: we wish to find its center of gravity. Now, it is evident that the volumes, V_1, V_2, etc., are too great to be considered elementary volumes for finding the center of gravity of V; for instance, we cannot assume that the center of gravity of V_1 is midway between *a* and *b*; because it may be $\frac{1}{4}$ or $\frac{3}{4}$ of the volume's length, D, from the first end, and the error would be serious. We must, therefore, find the center of gravity of each component volume. There are three ways of accomplishing this. The first is to divide the volume into short portions and assume the center of gravity of each to be midway its length. This requires immense labor, and is only approximate. The true position can be determined by the calculus; but a long formula must be used. The third method, and most common, is to approximate the center of gravity by a mental estimate. The first two of these methods are inconvenient; The first and last are inaccurate.

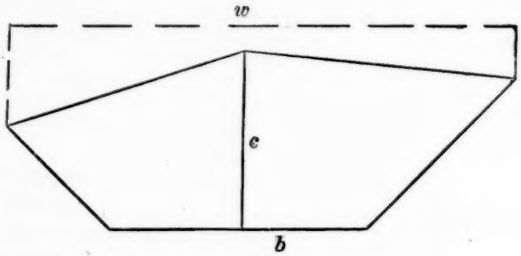
If we *might* assume each volume to be concentrated at a point midway between its ends, we could quickly find the center of gravity of the series, by ascertaining successively the centers of gravity of pairs of these points, disregarding figures in units and decimal places of volumes, till we should finally concentrate the entire weight at one point. If we cannot make this assumption, the labor of finding the center of gravity of each volume, by one of the above methods, must first be undertaken. In considering this problem, the writer has discovered an expedient, by which the foregoing assumption may be immediately made, and the error corrected by an exceedingly simple formula, that re-

mains the same whatever number of volumes compose the series.

First, to explain this, we must have an expression for the cross-sectional area. There is room for much choice here.

Let us consider the most ordinary shape of cross-sections, represented in the diagram, and assume the formula: for its area,

$$\tfrac{1}{2}\,w\,(c+sb)-sb^2,$$



from the writer's *Formulae for R.R. Earthwork;* the readers of which will recognize the expression, while others can readily deduce it by inspection of the values of the symbols in the diagram, S being the ratio of side-slope, equivalent to *vertical ÷ horizontal.* Considering this to be the area at a, at b we may likewise assume

$$\tfrac{1}{2}\,w'\,(c'+sb)-sb^2.$$

The area of a section at the distance x from a is, by the same formula,

$$\tfrac{1}{2}\left(w+(w'-w)\,\frac{x}{\mathrm{D}}\right)\left(c+(c'-c)\,\frac{x}{\mathrm{D}}+sb\right)-sb^2.$$

This, multiplied by dx, after performing the multiplication indicated, is the differential of the volume,

$$d\,\mathrm{V}_1 = \left\{\begin{array}{c} w(c+sb)dx \\ (w'c'-w'c-wc'+wc)\,\dfrac{x^2dx}{\mathrm{D}^2} \\ (wc'-2wc+w'c+sb[w'-w])\,\dfrac{xdx}{\mathrm{D}} \end{array}\right\} - sb^2\,dx.$$

By the principle of moments, using the calculus, x_1 being the distance of the center of gravity of V_1 from a, we have

$$x_1=\frac{\int_0^{\mathrm{D}} x\,d\,\mathrm{V}_1}{\mathrm{V}_1}=\frac{\int_0^{\mathrm{D}} x\,d\,\mathrm{V}_1}{\int_0^{\mathrm{D}} d\,\mathrm{V}_1}$$

$$=\frac{[wc+3w'c'+w'c+wc'+2sb(w+2w')-12sb^2]\,\dfrac{\mathrm{D}^2}{24}}{[w'c+wc'+2\,(w'c'+wc)+3\,sb(w+w')-12sb^2]\,\dfrac{\mathrm{D}}{12}}$$

$$=\frac{[w'c+wc'+2(w'c'+wc)+3sb(w+w')-12sb^2]\,\dfrac{\mathrm{D}^2}{24}}{[w'c+wc'+2(w'c'+wc)+3sb(w+w')-12sb^2]\,\dfrac{\mathrm{D}}{12}}$$

$$+\frac{[w'c'-wc+sb(w'-w)]\,\dfrac{\mathrm{D}^2}{24}}{[w'c+wc'+2(w'c'+wc)+3sb(w+w')-12sb^2]\,\dfrac{\mathrm{D}}{12}}$$

$$=\tfrac{1}{2}\,\mathrm{D}+\frac{[(\frac{1}{2}w'(c'+sb)-sb^2)-(\frac{1}{2}w(c+sb)-sb^2)]\,\dfrac{\mathrm{D}^2}{12}}{\mathrm{V}_1}$$

$$=\tfrac{1}{2}\,\mathrm{D}+\frac{\left(b-a\right)\dfrac{\mathrm{D}^2}{12}}{\mathrm{V}_1}=\tfrac{1}{2}\,\mathrm{D}+n.$$

If the center of gravity of the first volume be assumed at half its length, an error, represented by the distance n, is made. The effect of this error, on the average haul of the whole series, is the same as if the center of gravity of the series were moved a distance m, such that

$$m : n :: \mathrm{V}_1 : \mathrm{V}, \text{ or } m=\frac{\left(b-a\right)\dfrac{\mathrm{D}^2}{12}}{\mathrm{V}}$$

To compensate for this error, then, we must move the center of gravity of the series, as found by the assumption, a distance m, in the direction from a. By a process exactly similar, we find that the assumption that the center of gravity of V_2 is midway its length, may be correct-

ed by moving the center of gravity of the series a distance m_1, such that

$$m_1 = \frac{\left(c-b\right)\frac{D^2}{12}}{V}$$

A similar correction is found for the other volumes, till

$$m_4 = \frac{\left(g-e\right)\frac{D^2}{12}}{V}$$

Adding, we obtain

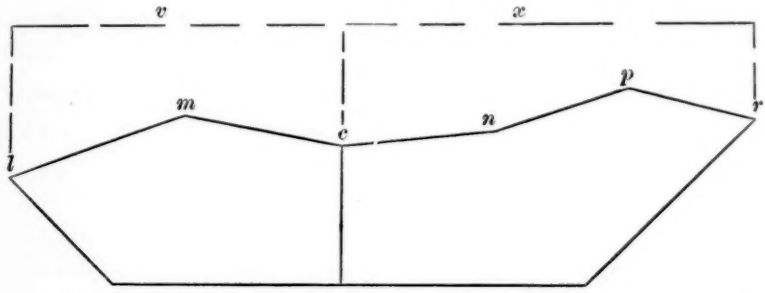
$$m+m_1+\text{ etc.}+m_4 = \frac{\left(g-a\right)\frac{D^2}{12}}{V} = M.$$

M is the total correction for the average haul, as first inaccurately determined by assuming the center of gravity of each volume to be half way between its ends. We may, therefore, consider each volume to be concentrated in its mid-section; or easily make the well known graphical or analytic solution of the center of gravity problem, and, finally, correct by the formula just derived.

If, instead of the formula for regular sections, we had used the formula for irregular sections,

$$\tfrac{1}{2}v(m+sb)+\tfrac{1}{2}x(p+sb)+\tfrac{1}{2}m'(c-l)+\tfrac{1}{2}n'(c-p)+\tfrac{1}{2}p'(n-r)-sb^2,$$

which represents the area of any irregular section (*Formulae for R.R. Earthwork*), v and x being the side-widths, m, n, p, the heights of breaks, m', n', p', their distances from center, we should have reached the same expression. Therefore, transverse irregularity does not alter the correction. In all cases, *subtract the area of first end from area of last, multiply difference by $\frac{1}{12}$ the square of distance between consecutive sections, and divide by total volume.* If the volume be in cubic yards, the numerator must also be divided by 27.



The same method may be applied to borrow-pits laid out between equi-distant cross sections.

Also, conceiving the first diagram to be a series of trapezoidal areas, the same method applies, and the corrected formula is identical, V being now the whole area, a the length of first ordinate and g the length of last.

We see by this correcting formula,

$$\frac{\left(g-a\right)\frac{D^2}{12}}{V},$$

that, if a piece of earthwork, between equi-distant cross sections, have the same end-areas, the center of gravity of each component volume may be freely assumed to be in its mid-section, and the resulting center of gravity of the series is perfectly correct.

It is also to be observed that, whatever be the difference between end areas, the error of the assumption is less as the square of the distance between sections. By the calculus this distance is reduced to dx, an infinitesimal of the second order, which, divided by the finite quantity V, gives zero for the correction. Hence the method of the calculus is perfect.

The centers of gravity of pyramids, prismoids, of all solids of revolution, whose generatrices are lines represented by equations of the first and second degrees, and of all segments of these between parallel planes, can be easily determined by this method. Thus, if h be the height of a cone, r the radius of its

base, we obtain for the distance of its center of gravity from the vertex,

$$\tfrac{1}{2}h + \frac{\frac{1}{12}h^3(\pi r^2 - 0)}{\frac{1}{3}h \cdot \pi r^2} = \tfrac{3}{4}h.$$

The center of gravity of the paraboloid, whose height is h, and the radius of whose base is r, is

$$\tfrac{1}{2}h + \frac{\frac{1}{12}h^2 \cdot \pi r^2}{\frac{1}{2}h \cdot \pi r^2} = \tfrac{2}{3}h.$$

The center of gravity of a hemisphere, whose radius is r, is, measuring from center of sphere, at a distance

$$\tfrac{1}{2}r - \frac{\frac{1}{12}r^3 \cdot \pi r^3}{\frac{2}{3}\pi r^3} + \tfrac{3}{8}r.$$

A NEW INVESTIGATION OF ONE OF THE LAWS OF FRICTION.

By A. S. KIMBALL, Professor of Physics in the Worcester Institute of Industrial Science.

From "The American Journal of Science and Arts."

REULEAUX, in the appendix to his recently published "Cinematics of Machinery," says that "many engineering schemes have failed because they were designed in accordance with the statements given in our text-books as the laws of friction." He furthermore adds, "that it is time that the experiments of Bochet and Hirn should be raised from their place as foot-notes to a position in the text."

During the last year, I have conducted experiments, on as extensive a scale as our laboratory would allow, for the purpose of settling, if possible, certain contested points in the doctrine of friction.

Our manuals of mechanics, following Morin and Coulomb, say the co-efficient of friction *does not vary with the velocity*. Bochet says that it *decreases as the velocity increases*. Hirn says that it *increases as the velocity increases*. Contradictory as these statements are, it is probable that each contains a partial truth. They need to be combined to make a complete statement.

The results of my experiments, which this paper is to describe, would indicate that the following is the true law, within the range of my experience. The coefficient of friction at very low velocities is small; it increases rapidly at first, then more gradually as the velocity increases, until at a certain rate, which depends upon the nature of the surfaces in contact and the intensity of the pressure, a maximum coefficient is reached. As the velocity continues to increase beyond this point, the coefficient decreases. An increase in the intensity of the pressure (the number of pounds on a square inch), changes the position of the maximum coefficient, and makes it correspond to a smaller velocity. The more yielding the materials between which the friction occurs, the higher is the velocity at which the maximum coefficient is found. Heating the rubbing bodies changes the position of the maximum coefficient to a higher velocity, since by heat the bodies are made softer, and are caused to yield to pressure with greater ease. For a considerable range of velocities in the vicinity of the maximum coefficient the coefficient is sensibly constant.

The experiments upon which I base my conclusions may be classified as follows:

(1.) Sliding friction down an inclined plane.

(2.) Sliding friction at uniform velocities on a horizontal plane.

(3.) Friction of belts on the surface of cast iron pulleys.

(4.) Friction of wrought iron journals in boxes or bearings of different materials.

(1.) *Sliding friction down an inclined plane.*—A full description of the apparatus used, and some of the results obtained, will be found in this Journal, March, 1876; also in VAN NOSTRAND'S ECLECTIC ENGINEERING JOURNAL, June, 1876. It is sufficient for my purpose to say that the sliding body was made to carry a smoked glass, upon which was traced a wave line, which by direct measurement gave the time of sliding and the spaces passed over, from which it was easy to compute the corresponding coefficients of friction. In the article referred to, no velocities less than two

feet a minute were examined. An extension of the same experiment, to the case of much lower velocities, showed a curve concave toward the time line, indicating that at these velocities the coefficient of friction was increasing. As the velocities increased, the line changed its direction and became convex toward the time line; thus giving in the limits of one experiment a verification of the statements made above. For further particulars respecting this method of experiment, I refer to the article published in March of last year.

(2.) *Sliding friction at uniform velocities on a horizontal plane.*—A heavy pine plank, fifteen feet long, whose surface had been planed, was carefully leveled on the floor of the laboratory. The weight-box was mounted upon shoes which could be covered with the material experimented upon. To its forward end a spring dynamometer was attached, which was pulled by a cord wound around a drum, which was made to revolve at a constant velocity. The motive power was a fifteen horse-power Corliss engine, belonging to our machine shop, whose fly-wheel runs with great regularity at the rate of sixty revolutions per minute. A shaft from the shop runs underground to the cellar beneath my laboratory, whence through several countershafts the power is transmitted to any part of the room. By means of change pulleys, I can easily command a great range of velocities.

The experiments were made by drawing the box along the plane at various velocities, and reading the friction from the dynamometer. This combination answers very well for low velocities, but the slide can not be easily stopped when the speed is great.

Several series of experiments were made, with wood on wood, also with leather on wood. The results verify the first part of my statement, that the coefficient of friction increases with the velocity, when this is small. The following are some of the results obtained by this method of experiment: (*next column*)

These experiments show the increase of friction with the velocity at low speeds quite clearly; and in connection with the series published last March, which showed its decrease at high velocities, would prove the high probability of the law as I have stated it, especially since the experiment on the inclined plane has been made to show a variation of the coefficient from a low value, through a maximum, to a low value again, while the velocity constantly increases.

TABLE I.—PINE ON PINE. SLIDE LOADED WITH 100 POUNDS. VELOCITY IN INCHES IN A MINUTE.

V.	Coefficient of Friction.
5	.19
11	.20¾
75	.24
300	.25

TABLE II.—LEATHER ON PINE. SLIDE LOADED WITH 100 POUNDS. VELOCITY IN INCHES IN A MINUTE.

V.	Coefficient of Friction.
.79	.22
1.58	.27
3.94	.33
9.98	.36
29.14	.38
72.50	.41
157.50	.43
226.80	.45
300.	.46
466.	.47½

(3.) *Friction of belts on the surface of cast iron pulleys.*—A piece of leather belting was hung over a cast iron pulley. To one end a determined tension was given by a fixed weight; to the other end was attached a spring dynamometer. The tension of the ends of the belt being known, the coefficient of friction was easily found. Several pulleys were used, and various kinds of belting; and a considerable range of tensions was employed, with uniform results.

Two tables are here given, one selected to show the increase of the coefficient at low speeds, the other to show the existence of a maximum coefficient at a definite velocity. In the third table, the first column gives the velocity in feet in a minute; the second and third give the tensions of the ends of the belt; and the fourth gives the relative values of the coefficients found, the maximum in each case being represented by 1.00. I give relative values, since they show variations more clearly than the absolute values. (*See Tables on following page.*)

(4.) *Friction of wrought iron journals in boxes of different materials.*—In this course of experiments a modification of the friction brake was used. A description of the arrangement in one series will serve for all the others. A shaft 1″ in diameter was adjusted so that

TABLE III.

V.	T_2.	T_1.	C.
.37	30	13	.42
.52	30	12½	.44
1.1	30	11½	.48
2.3	30	10½	.53
2.9	30	10	.55
4.4	30	9½	.58
15.4	30	6½	.78
34.1	30	5½	.86
80.3	30	4½	.96
104.5	30	4¼	.99
228.8	30	4¼—	1.00

In the following table only velocities and relative coefficients are given:

TABLE IV.

V.	C.
18	.82
92	.93
660	1.00
1190	.96
1980	.82
2969	.69

it could be driven at almost any rate between one revolution in two days and 1,000 in a minute. A hole was bored through a block of cast iron $3\frac{1}{2}'' \times 3\frac{1}{2}'' \times 1\frac{1}{2}''$, and carefully fitted to the shaft; rigid iron rods were screwed into the top and bottom of this block, and adjusted to stand in a vertical line at right angles to the shaft. Upon these rods slotted weights could be placed, and thus the pressure upon the shaft and the center of gravity of the brake could be readily adjusted. Upon the front of the block a plane mirror was fastened, and before it, at a convenient distance, were placed a scale and telescope. When the shaft was turned, the friction between it and the brake caused the latter to turn until the moment of the friction was equal to that of the brake, and the angle at which this equality was established could be read from the scale by the telescope. As the center of gravity was always adjusted so that the brake never revolved through an angle of more than three degrees, the scale readings were approximately proportional to the coefficient of friction; and since relative and not absolute results were sought for, the labor of reduction was not undertaken. Several tables will be given to illustrate the method of conducting a series of experiments with this apparatus.

The results given above were made with high velocities, and show coefficients of friction decreasing as the velocity increases.

TABLE V.—WROUGHT IRON SHAFT, 1″ DIAM.; BOX, CAST IRON, $1\frac{1}{2}''$ LONG; LOAD, 100 LBS. SHAFT WELL OILED.

Velocity of the circumference of the shaft

	No. 1, 72″.	No. 2, 272″.	No. 3, 605″.	No. 4, 1320″.
Scale readings	515	500	...	...
	515	500	...	...
	515	495	...	...
	515	495	...	...
	515	495	...	...
	515	495	...	...
	515	...	485	...
	515	...	485	...
	520	...	485	...
	520	...	485	...
	520	...	485	...
	520	....	490	...
	525	...	...	480
	525	...	...	480
	525	...	...	480
	525	...	...	475
	525	...	...	480
	525	...	...	485
Mean	519	497	486	480
Position of equilibrium.	464	464	464	464
Deflections	55	33	22	16

Relative values of the coefficient of friction.

No. 1, 1.00, No. 2, .60. No. 3, .40. No. 4, .29.

The results of a similar series with very low speeds are given in the next table:

TABLE VI.

Velocity of the circumference of the shaft: No. 1, .007″; No. 2, .027″; No. 3, .060″; No. 4, .132″ in a minute.

No. 1, .37. No. 2, .51. No. 3, .73. No. 4, 1.00.

These results, unlike those of the former table, show a coefficient increasing as the velocity increases.

A large number of experiments similar to those given above, have been made with uniform results.

I have been able to verify experimentally the law stated early in this paper in the following cases; wood sliding on wood, wood on iron, leather on iron, zinc on iron, and copper on iron; and to obtain results verifying the first half of the law in the case of leather on wood.

The experiments above detailed make it easy to explain the various results obtained by the three authorities quoted at the beginning of this paper. Morin experimented under conditions which gave him a coefficient very near the maximum, and thus his results are approximately constant. Bochet experimented with railway trains, his conditions were high

speeds, hard rubbing surfaces, and great intensity of pressure. All these circumstances are favorable to the result he obtained, namely, a coefficient decreasing as the velocity increases. Hirn, on the other hand, employed very light pressures, less than two pounds on a square inch, and kept his rubbing surfaces so thoroughly lubricated that the friction was between oil and oil instead of two metal surfaces; his speeds were not very great. These conditions are precisely the ones I have found favorable to the results he reached,— a coefficient, increasing as the velocity increases. It would be very easy to form a theory which would account for the variation of friction with the velocity, under the rule I have given.

It is well known that a given deflection in a bar is produced by a weight acting for five seconds, for example, that the same deflection may be produced, by a less weight acting for a longer time. Now, as the force required to overcome friction is, partially at least, expended in bending down the minute irregularities on the surface of the rubbing bodies, it becomes evident how, other things being equal, a rapid motion would call for the exertion of a greater force than would be required if the motion were slow.

On the other hand, the longer two surfaces under pressure are in contact the greater must be the interlocking of the irregularities upon the rubbing surfaces. On this account a rapid motion would not require the expenditure of so great a force to overcome the friction. Thus, we have two effects, varying with the velocity, but having opposite signs. Now it is not probable, from the nature of the case, that these effects are numerically equal, or even proportional, and thus we can, at the least, say that the conditions are favorable to the existence of a maximum resultant effect. Having, however, ascertained the fact by experiment, the explanation becomes a matter of minor importance.

It may be said that these facts have no practical importance at the velocities ordinarily employed. I would call attention to Table V, where it will be seen that by increasing the velocity of shafting within the limits of ordinary shop practice, a reduction of the coefficient of friction of quite fifty per cent. may be made. The pressure on the shaft might also be reduced, for it would be unnecessary to maintain so great a tension upon the belts, and thus in some cases a very considerable economy of power might be effected. We know that in many shops a large fraction of the power developed by the engine is expended in overcoming the friction of the shafting and machinery.

On the other hand, it is desirable that the friction of the belt upon the pulley should be as great as possible. The conditions usually met with, which determine the friction of belts, are, low intensities of pressure, a rubbing surface which yields with considerable ease. In such cases a high speed is needed to develop the greatest amount of friction. See Tables III. and IV.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.— Among the recent papers presented to the Society are:—Brick Dams, by Edward P. North C.E.: and Approximate Determination of Stresses in Eye-Bar Heads, by W. H. Burr C.E.

This latter paper forms the chief part of the May number of the Transactions.

We shall present an abstract of it in some number of the present volume.

IRON AND STEEL NOTES.

New Process for Making Steel.—The Red Moss Metal Company, Warrington, have been some years developing a method of making steel direct; and having succeeded so far as to get the new steel in considerable quantities, and with highly satisfactory results, into the market, we propose to give a short account of the process, principally in the words of Mr. Larkin, the inventor of the process, and to add a few supplementary statements of what we ourselves have seen of the company's works, and the steel they are daily producing. The initial efforts of the company were directed to the magnetic iron-sands as the most convenient for their operation. These, however, have been for some time, and for commercial reasons chiefly, abandoned in favor of ore from Marabella on the south coast of Spain. The large and small lumps of this ore first passed through the jaws of a Blake's crusher, set as closely together at the bottom as practicable, and the crushed material is sifted as it falls. The coarser portion is then passed through a disintegrator. In this way the whole bulk of the ore is very cheaply and readily reduced to the condition of the iron-sand already described. But of course the gangue of the ore is crushed equally with the ore itself; and the next step is to separate the actual ore from all such extraneous matter, and get as nearly as

possible the pure oxide of iron. This is very effectually done by means of a self-acting magnetic separating machine, specially devised for the purpose, and capable of dealing with large quantities of material. In this machine the particles of magnetic oxide are picked up by magnetic attraction, and carried in their proper receptacle, while the refuse is safely deposited in another. Having thus got as pure and rich a material as possible in a powdered condition, the next operation is to thoroughly mix with it a sufficient quantity of powdered carbonaceous matter to combine with the oxygen of the ore, and thus effect its reduction. The carbonaceous matter used consists of powdered charcoal and powdered resin, or other suitable bituminous substance, the two being reckoned together somewhat in excess of the oxygen to be removed. This mixture of powdered ore and carbonaceous powders is slightly warmed, and compressed into bricks in an ordinary brick press, and will then be ready for the reducing furnace. The reducing furnace consists of a series of D-shaped gas retorts, with doors to open at each end. These retorts are heated by a fire acting somewhat upon the principle of a Siemens' gas producer, and are thoroughly supported throughout their entire length by an intricate arrangement of brickwork, which also serves to prevent a too ready escape of hot air into the flue. The burning gases from the fire are also made to completely envelop the retorts by being carried over and under in a zigzag way, thus still further delaying their passage and arresting the heat with which they are charged. Air-holes are opened at regular intervals in order to complete the combustion of the gases as they circulate around the retorts, thus securing the greatest heat where it is actually wanted, and also securing complete combustion of the fuel used. The consumption of the smoke is perfect. There are other points of importance in connection with the furnace, but as they would not help to a clear idea of the method as a whole they may be omitted. Let the reader now imagine one of these retorts at an average working heat, empty, and ready to be charged. The door being removed from the feeding end of the retort, a small stack of pressed bricks, consisting of ore and carbonaceous matter, and of bulk to fill the section of the retort, is closely packed on a rectangular iron plate and pushed into the further end by means of an iron rod. The plate is then withdrawn, leaving the small stacks of bricks securely placed. A second and third feed immediately follow, filling the retort, which is at once closed. After having been exposed to a pretty full red heat for nearly twenty-four hours, gas will have ceased to be given out, the carbonaceous matter will have become practically consumed, and the oxide of iron will have become converted into red-hot iron powder. The next problem is how to convey this red-hot powder from the retort without exposure to the atmospheric air, and to keep it so till it is cold. A charge is now supposed ready for removal. Ordinary coal gas is first, by means of pipes provided for the purpose, turned on into the inside of the discharging end of the retort, in order to produce a full outward pressure of gas while the discharging door is removed, the door being at the underside of a projecting end-piece of the retort. The door being thus removed, an iron receiver is brought up closely under the projecting end-piece, and securely supported there. By a similar arrangement of pipes, gas is now let also into the inside of the feeding end of the retort, when the door of that end is quickly removed, and a temporary door with a wide slot half way down the middle of it is put in its place. The slot is for the introduction and working of the discharging tools, by which the red-hot powder is quickly pushed forward into the receiver placed at the discharging end. As soon as the retort is empty, the gas at both ends is turned off, and the iron receiver containing the metallic powder is removed and kept carefully closed until its contents are cool. When the metallic powder is sufficiently cooled down, and no injury can arise from its exposure, it is turned out of the receiver, and again passed through the disintegrator and the magnetic machine for a final purification. Thus by a few simple, and almost self-acting operations, requiring little more than faithful attention and accuracy in weighing and mixing, the steel maker is able to produce pure metallic powder. For the production of tool-steel the operator mixes with the metallic powder (besides some small percentage of flux) whatever additional amount of carbon may be needed, chiefly in the form of resin. This resin easily makes it possible to compress the finished powder into solid cakes, in the same way as the bricks of ore and charcoal were compressed in the first instance. The cakes of finished material are then stacked up, ready to be melted in crucibles in the usual way, with the addition of manganese or any other alloy that may be found advantageous.—*Iron.*

RAILWAY NOTES.

In 1867 experiments were made with iron cross rods in substitution for oak sleepers on the Nassau Railway, a stretch of seven and a half miles, from Oberlahnstein to Ems, being the first line laid with them. They have stood well, and the Lahn line is now being laid with them, on Hilf's system. Experiments have been made on other lines with, it is reported, complete success. The Belgian lines, as laid with Vignoles rails and oak sleepers, cost the State 3 fr. 28 c. per current meter per annum for maintenance, and 33 fr. 66 c. for first laying. On Hilf's system the State would have to expend 2 fr. 12 c. in maintenance, and 33 fr. 63c. per meter running first cost. The gain to the state would be thus 62 centimes per meter per annum. These calculations are on the basis of the duration of oak sleepers not exceeding ten years, while it is shown on the other hand that pine sleepers, treated with sulphates, will last fifty. The Germans are still complaining of the high rates they have to pay for the carriage of heavy goods. The addition made last August to rates then current, was as high as 20 per cent., and dear carriage makes competition with foreign makers the more

difficult. The increase in outlay on carriage alone naturally becomes a very serious item, now that competition is so exceptionally close. One establishment alone, the Hord Smelting Works, lost—we may perhaps venture to say—close on £2000 last year by the operation of this increased percentage. How to lower freights is a question, the solution of which, in the opinion of many Germans, is possible only by the purchase of all the railways by the State, and their management by the State on the public behalf. As is well-known, this idea is finding favor with the heads of the paternal Government of Germany, and it is confidently expected that on the next assembly of the Prussian Landtag, a proposal in that sense will be laid before it. Coal is active, both in Westphalia and the Saar district. Last year saw a rise in production in both of 5 and 6 per cent. respectively.

ENGINEERING STRUCTURES.

THE BERGEN TUNNEL.—The second tunnel under Bergen Hill, begun in the fall of 1873, has just been completed. This tunnel is at the eastern terminus of the Delaware, Lackawanna and Western R.R. The tunnel has been made under the supervision of James Archibald, of Scranton. The tunnel is 27 feet wide in the clear and 18 feet over top of iron clear; its entire height is 20 feet and 7 inches. From end to end the bore has been through Dolerite, commonly known as "trap rock," a stone which is said to be hard to drill but easy to blast. For four fifths of its length it is brick-arched. It is ventilated by seven shafts, all of which are brick-lined, with one exception. Three are elliptical, opening the full width of the tunnel, and 8 feet wide; one is 16½ feet by 7 feet; two are 6 feet in diameter, and another, opening the full width of the tunnel, is 12 feet wide. With a west wind it is claimed no inconvenience will be felt by passengers through the tunnel from smoke and steam, the shafts serving as excellent chimneys with the wind in this quarter. The cost of the excavation and shafts was $800,000, of the brick arching $105,000.—*Railway Review.*

TESTING THE NIAGARA SUSPENSION BRIDGE.—The suspension bridge over the Niagara river was constructed by John A. Roebling, in 1855. It has a span of 821 feet, and a deflection of 59 feet; 14,560 wires are employed in the cables, the ultimate strength of which is 12,000 tons. Since its completion, in the year mentioned the bridge has been subjected to the almost constant strain of heavy railroad trains; and thus for a period of 22 years it has undergone a trial of the greatest severity. Quite recently it was deemed advisable to overhaul the structure thoroughly in order to determine whether any repairs were required, or whether the jarring or straining to which the wires had been submitted had—as some theorists believed possible—impaired the quality or tenacity of the iron. Accordingly the bridge was closed to traffic; and to Colonel William H. Paine, Assistant Engineer of the East River bridge, was assigned the duty of critically inspecting the structure. Colonel Paine has sent to the *Scientific American* a short account of his investigations, which have resulted in his conclusion that the safety of the bridge is in no wise lessened. The anchorage cables were imbedded in masonry and cement, which it was necessary to remove in order to admit of their examination; a task of no small difficulty, as the masonry was like solid rock. It was found that out of the 14,560 wires less than a dozen were seriously corroded, and these were in the first anchorage. The metal on the other wires showed the original grain with distinctness. Not content, however, with this highly-favorable appearance, Colonel Paine proceeded to experiment upon the wire, in order to discover whether the means provided to allow of its expansion and contraction—namely, the placing of the bed-plates which receive the cables on top of the towers, on rollers—had been sufficient to prevent the longitudinal stress upon the filaments destroying their elastic quality. An apparatus was used capable of marking a stretch to 1-10,000th of an inch; and this being adjusted, a heavy freight train was moved upon the bridge. The elongation of the wire was found to be very nearly equal to that which the formula, used by engineers for ascertaining such results, showed the stretch of a perfect cable, similarly made, should be under like strain: so that not only had twenty-two years' service not resulted in any corrosion of the wire, but the elasticity of the same under the enormous strains had not been impaired. In the second experiment, a single wire from one of the main cables was tested. A strand of 520 wires was selected, and the binding removed, so that every wire was perfectly free. Three wires were then chosen, and across them a knife mark was made. The middle wire of the three was cut at the mark; and on testing it by the delicate instrument above noted, it was found to have contracted to within a small fraction of what it should have reached when relieved from its portion of the weight of the bridge. In experiment No. 3 a single wire was detached and weighted until it broke.

The object here was to see whether the nature of the metal had been altered; for if the iron had become granular and had lost its cohesiveness, the fracture would be a straight one, similar to that of cast iron. On the contrary, the wire, when subjected to the stress, extended until its diameter was reduced fifty per cent. before it broke. A cable guy was next selected and made to sustain a dead weight. The construction estimate places the maximum burden which that guy would ever be required to bear at fifteen tons. The guy parted at fifty-three tons. To show how perfectly the wire had retained its original characteristics, Colonel Paine, having observed that the wire, on being removed from the cable, tended to coil in a circle of about five feet in diamater, sent to the person who had originally prepared the wire for its place for information as to the size of its former coil. He learned that it had been wound on a drum two feet in diameter, and that it had been subjected to a straightening process which took about half the curve out of it. Certainly no more remarkable proof could be adduced to

show that the spring of the wire had in no respect been overcome. Although the bridge has thus been shown to be thoroughly safe, and to have wonderfully withstood wear, still more elaborate examinations are to be made, and the structure will not be open to traffic until these are completed.--*English Mechanic.*

ORDNANCE AND NAVAL.

TORPEDOES AT SEA.—A very curious experiment has been tried off Cherbourgh, with one of Messrs. Thorneycroft's torpedo boats. This is a little gray craft, which floats with her deck close to the water-level, and is capable of steaming at 18 knots an hour. She carries a powerful torpedo at the end of a long spar. Admiral Jaurez, commanding the French Channel Squadron, had an old craft, the Bayonaise, taken in tow by a powerful tug, and run out to sea. The torpedo boat, manned by Lieutenant Lemoinne and three others, started in pursuit, and after a little time overtook the Bayonaise and ran into her. The torpedo exploded immediately, and the Bayonaise went down like a stone. Two days later the experiment was repeated with the same success on another ship. No one doubted that an old wooden ship could be sunk in this way, or probably an ironclad; but there was great room for doubt whether the torpedo boat and her crew could sustain the shock of the explosion. The Cherbourg experiments are eminently useful, as demonstrating that torpedo boats can be used apparently with fair safety, that is to say, they will not injure themselves. It would be quite possible to over-estimate the worth of this system of attack, however. An ironclad having the power of depressing her great guns sufficiently would in the day-time wait quietly until the boat was within a few hundred yards, and then with one or two shots dispose of her. But at night the attack of such a foe would be a different matter.

BOOK NOTICES

THE ART OF ELECTRO-METALLURGY. By G. GORE, F. R. S. London, 1877. For sale by D. Van Nostrand. Price $1.50.

This work presents fully the theory of Electro-deposition and a description of all known processes.

The use and management of magneto electric machines is fully treated.

A fair supply of figures illustrates the text.

The book is the last addition to the series of Text Books of Science.

HAND BOOK OF ELECTRICAL DIAGRAMS, Second Edition. By CHARLES H. DAVIS & FRANK B. RAE. New York: D. Van Nostrand. Price $2.00.

The first edition of this book is only a year old.

In a series of plates, thirty in number, all the important electrical connections employed in telegraphy are in the fullest manner illustrated.

The plates are exceptionally neat and the small amount of descriptive text is compactly written and sufficiently supplements the illustrations.

INTERNATIONAL LAW. By JOHN A. DAHLGREN, late Rear Admiral U. S. N. Edited by CHARLES COWLEY. Boston: For sale by D. Van Nostrand. Price $1.50.

This book presents the following topics. Biographical Sketch of the Author; Law of Blockade; Contraband of War; Visitation and Search; Duties of Naval Commanders of Foreign Stations; Addenda by the Editor.

THE STEAM ENGINE. By F. J. BRAMWELL, F. R. S. London: Macmillian & Co. For sale by D. Van Nostrand. Price 25 cts.

This is an illustrated pamphlet of the Science Lecture Series, prepared for the South Kensington Course.

The discussion of the action of steam for different degrees of expansion is quite complete.

There are thirteen illustrations.

THE ARCHITECT'S GUIDE. By FREDERICK ROGERS, Architect. London: Crosby, Lockwood & Co. For sale by D. Van Nostrand. Price $3.00.

This is a work for the master builder rather than the professional architect, and is designed to supply deficiencies in rudimentary education. There are many useful tables compiled from various sources.

The typography and cuts may be ranked between indifferent and poor.

FIRE PROTECTION. By EYRE M. SHAW, Chief Officer of London Fire Brigade. London: Charles & Edwin Layton. For sale by D. Van Nostrand. Price $6.00.

This is a manual of organization, machinery and discipline of the Fire Brigade of London.

The elaborate description of fire-escapes and their management is a prominent feature of the work, although nothing seems wanting that pertains to the duty of the firemen in any emergency.

THE ALKALI TRADE. By CHARLES THOMAS KINGZETT. London: Longmans, Green & Co. For sale by D. Van Nostrand. Price $6.00.

This is a new contribution to practical technology. A complete description of the processess of manufacture of the salts of the alkalies is given accompanied with abundant illustration of the necessary machinery.

PROCEEDINGS OF THE INSTITUTION OF CIVIL ENGINEERS.—We have lately received the following:

Experiments and Observations on the Emission of Heat by Hot Water Pipes. By Wm. Anderson, C E.

Also Canadian Narrow-Gauge Railways. By Edmund Wragge C.E.

THE FORCES OF NATURE. By AMIDIE GUILLEMIN. Translated from the French by Mrs. Norman Lockyer. Part I. Illustrated by nearly 500 engravings.

This is a voluminous work on general Physics of most attractive appearance. Judging from the portion before us, it will be when complete, an indispensable compend of such scientific subjects as we are accustomed to group under the title of Natural Philosophy. It will be a complete reference book for the general reader.

A PRACTICAL TREATISE ON LIGHTNING PROTECTION. By HENRY W. SPANG. With Illustrations 12 mo. cloth, $1.50. Claxton, Remsen & Haffelfinger. For sale by D. Van Nostrand.

This book aims to demonstrate that the electricity of the earth is principally accumulated in the subterranean water-bed, and shows that nearly all the lightning rods or conductors now erected cannot be relied upon for the easy passage of heavy lightening discharges, owing to the small quantity of metal they contain and the dry condition of the earth around and beneath their lower terminals.

It gives plain and explicit directions for the proper protection of buildings of every description, ships, oil-tanks, steam-boilers, wooden bridges, telegraph poles, etc.

The author insists that the metal roofs, rain and gas pipes, iron fronts, stacks, etc., about buildings are much better conductors of lightning discharges than the lightning rods with polished points, etc., as heretofore employed, and if properly connected with the earth will effect absolute protection.

A simple and reliable method is explained, by which the rain and waste water can be utilized to always maintain an easy path in the earth for lightning discharges, and thereby enable buildings and other structures to be properly protected.

REPORT OF CHIEF ENGINEER J. W. KING, UNITED STATES NAVY, ON EUROPEAN SHIPS OF WAR AND THEIR ARMAMENT, NAVAL ADMINISTRATION AND ECONOMY, MARINE CONSTRUCTIONS AND APPLIANCES, &c. Washington: Government Printing Office.

This is a very remarkable book, which we have read with some astonishment, and just a little dismay. In July, 1875, Mr. J. W. King, Chief Engineer of the United States Navy, and late Chief of the Bureau of Steam Engineering, was instructed by his Government to proceed to Europe and collect information concerning Old World navies. He left New York in Aug., 1875, and did not return to the States until July, 1876. The volume before us has just reached this country, and is Mr. King's report on what he saw and what he was told. The most noteworthy feature of the work is, that it contains more minute and copious information regarding the British Navy than any other book in any language. Indeed, the volume ought to be in the hands of every officer of our navy, for it contains information which very few of them possess; and we confess that while we feel alarm at the publication of such a work in a way that supplies information concerning every detail of the strength of our ships to foreign Governments, we also feel some annoyance that all this information has been carefully kept back from Englishmen. Is it not strange that the best book of reference on the British Navy should be the report of an American Engineer to his government? The policy which we deprecate has, however, nothing to do with the merits or demerits of the work before us; and leaving its consideration, we may proceed at once to say that Mr. King has done his work thoroughly well; his style is concise and lucid, and as a text-book of the British navy, nothing can be found to compare with the volume before us. Of the two hundred and seventy-three large octavo pages of which the work consists, no fewer than 115 are devoted to describing our ships of war, while much of the remainder of the work is occupied with the discussion of questions almost entirely concerning English systems of constructing engines, &c. The information supplied concerning the navies of France, Russia, Germany, &c., is very meagre; no doubt Mr. King did not find that all that he asked for was placed at once and without hesitation into his hands. The work is illustrated with very effective diagrams, none of which, so far as we are aware, have been published before. Thus for example, the hydraulic loading gear of the Thunderer's guns is illustrated and minutely described.

A chapter is devoted to each of our best and most recent ships, and each chapter contains a plan and an elevation of the vessel, showing the arrangement of her armor, armor-plated deck, turrets, and guns. A description of the machinery of the ship is contained in each chapter, and we may say shortly that these descriptions are admirably written. Mr. King deals but little in criticism; and when we have said that the work is an accurate and complete description of the most powerful men-of-war in the world, and that in this respect it stands alone, we have said all perhaps that need be said. We cannot conclude without warning our readers that as this is a Government publication some difficulty may be experienced in obtaining it in this country.—*Engineer.*

STRENGTH AND DETERMINATION OF THE DIMENSIONS OF STRUCTURES OF IRON AND STEEL, WITH REFERENCE TO THE LATEST INVESTIGATIONS. An elementary appendix to all text books upon iron and steel constructions: by Dr. Ph. JACOB J. WEYRAUCH, Professor in the Polytechnic School at Stuttgart. Translated by A. JAY DUBOIS, Ph. D., Professor of Civil and Mechanical Engineering, Lehigh University. New York, John Wiley & Sons; also another translation published by D. Van Nostrand.

[From "The Railroad Gazette."]

Reading the title of this book and taking a first glance at its contents we experienced a feeling of pleasure and satisfaction that the time had come when theorists, after so many brilliant mathematical fireworks, begin to appreciate better the hard, slow work of practice and experiments, and as it were to return from the realms of hypothesis to reality and truth.

But examining the entire book carefully, we were sorry to observe that its title promises more than its thirty chapters realize. With pleasure we admit that Professor Weyrauch has done his best to collect data from the labors of Kirkaldy, Styffe, Woehler, Bauschinger, Thurston, etc., and also to study the views of a great number of commentators on old and new experiments, as evidenced by numerous references, constituting by no means the least valuable part of the book. Herr Weyrauch has touched many questions which undoubtedly may lead to further valuable experimental re-

searches. In the course of his deductions he could not help letting us see how many formulæ and theories of constructive design, though satisfactory to the student, are nevertheless based upon wild hypotheses, but little in reality we know about the properties of material, and how much less about the details of construction.

If these facts present themselves clearly to the reader of Herr Weyrauch's book, and if many will read it, Herr Weyrauch has done a good work. But the new book can hardly be considered an appendix to *all* text books upon iron and steel construction, and it appears to us that Professor Dubois, who is a literal translator of German idioms, would perhaps do still more good if he would translate into German the second volume of the theory of strains by B. B. Stoney. This volume of 268 pages, with the exception of Woehler's and Bauschinger's experiments, contains at least as much real knowledge as the book now presented, and it is more suited to be an appendix to German text books than that of Herr Weyrauch is adapted to be an appendix to English or American ones.

The English Iron Commissioners of 1849 made experiments on impact and repeated strains of cast and wrought-iron bars, and found the "law" that repeated strains below the ultimate strength cause rupture. These experiments were not further followed up in England, except that Fairbairn—about the time that Woehler began his experiments proper, namely, in 1860—tested a riveted plate girder under repeated flexures. Mr. Fairbairn discovered that this girder, whose strains per square inch he probably computed too low (he did not add the concentrated rivet strains, the girder being loaded with a single load in the center), broke after a comparatively small number of repetitions (3,463,000) of the strain of 18,600 lbs. per square inch.

Mr. Fairbairn did not describe the pitch of the rivets used, so that we cannot judge how great the strain per square inch actually was.

Herr Woehler, being Engineer of the shops of the Silesian Railroad in Prussia since 1858, began to investigate the strains *actually* sustained by the axles of railroad freight and passenger cars.

Preliminary results thus obtained were given in the Berlin official engineering periodical (*Zeitschrift fuer Bauwesen*) in the year 1860, and then presented a number of most interesting facts, worthy of careful study. Herr Woehler continued the experiments, got up new apparatus for testing axles under as nearly as possible the same strains as they have to sustain in practice, and, the results being very interesting, extended his labors to repeated flexures in one direction and in both directions, under a permanent strain with superposed variable strains, under repeated extensions, etc., etc. The results are clearly and modestly set forth and are well interpreted in the volumes of 1863, 1866 and 1870 of the Berlin periodical quoted.

These reports in part were translated and published in England and in America. They were frequently quoted and for quite a number of years have influenced the views held by our designers, by whom they probably were at first more appreciated than even in Germany. Experiments somewhat similar to those of Herr Woehler were directed by Stummer, of Austria, and Herr Woehler, having been called to other duties, arranged to have an engineer charged with the continuance of his own.

This labor was transferred to Professor Spangenberg, who made a report in 1875, a translation of which has appeared in Van Nostrand's "Science Series." The book written by Herr Weyrauch is principally founded on Woehler's results, or rather on a formula which was believed by Professor Launhardt to embrace these results. One would think that especial pains would have been taken to prove the stability of this formula which has to support Professor Weyrauch's mode of calculation of sections of bridges and similar structures. But Professor Weyrauch has not given or interpreted Woehler's specific experiments, and he has failed to bring sufficient proof of the formula on which he builds.

The old method of proportioning the parts of a structure by adopting one or two maxima strains per square inch, and therefore calculating the sections singly from *the maximum* strain in a member, has been known to be crude for some time. In fact, at the time of the famous English Iron Commission it was proposed to use a higher (double the) "factor of safety" for variable than for static stress. The late Professor Rankine also recommended this method; it was used in England and elsewhere; it was embraced in a modified form by the Engineer Gerber of Munich; it has been recommended in this country, and many years ago Mr. Shaler Smith in a pamphlet showed how to take into consideration the *frequency* of the maxima strains *and* the distinction between *static* and *live* strains.

It is known that car springs are exerted to calculated statical strains as high as 100,000 lbs. per square inch, and that the motion of the cars increases this strain not inconsiderably on rough parts of the road. Woehler examined the properties of such springs by putting steel bars under the action of constant flexure increased by variable flexures similar to those caused by motion.

Thus far, then, Professor Weyrauch tells us nothing new. But what is new is his somewhat pedantic application of Professor Launhardt's hypothetical formula to subjects inaccessible to mathematical reasoning, like riveting.

We will now briefly give the material which has led to Launhardt's formula and to Professor Weyrauch's book and to the translation before us:

Of Herr Woehler's eighteen tables of experimental results, we here give only Table IX., which is intended to form the basis of Launhardt's formula: (*See following page.*)

The manner in which the formula was derived from these figures is about this: Considering experiment No. 17 of this first series, it is *assumed* that 55,000 lbs. is the maximum variable strain which will just leave a bar durable for infinity. From experiment 9 of the second series it is further *judged* and *assumed*

EXPERIMENTS ON BARS, BENT IN ONE DIRECTION WITHIN FIXED STRAINS.

Bars of crucible spring steel (by Krupp).

No. of Experiments.	Maximum Strain per sq. in.	No. of Flexures Causing Rupture.	Remarks.
11	110,000	39,950	Steel not hardened.
12	99,000	72,450	
13	88,000	132,650	
14	88,000	117,000	
15	77,000	197,400	
16	66,000	468,200	
17	55,000		Not broken after 40,600,000 repetitions.
18	49,500		Not broken after 32,942,000 repetitions.

No. of Experiments.	Strains limited between—Pounds per square inch. Maxima.	Minima.	No. of Flexures Causing Rupture.	Remarks.
1	132,000 and	33,000	22,900	Hardened.
2	132,000 and	44,000	35,600	
3	132,000 and	55,000	86,000	
4	132,000 and	66,000	191,100	
5	132,000 and	77,000	50,100	
6	132,000 and	77,000	251,400	
7	132,000 and	88,000		Not broken after 35,600,000 repetitions.
8	132,000 and	99,000	33,878,700	
9	110,000 and	18,300	62,000	Not hardened.
10	110,000 and	36,666	149,800	
11	110,000 and	55,000	400,050	
12	110,000 and	64,100	576,700	
13	110,000 and	72,690		Not broken after 19,673,300 repetitions.
14	99,000 and	22,000	81,200	Not hardened.
15	99,000 and	33,000	156,200	
16	99,000 and	44,000	225,300	
17	99,000 and	55,000	1,238,900	
18	99,000 and	55,000	300,900	
19	99,000 and	66,000		Not broken after 33,600,000 flexures.
20	88,000 and	11,000	99,700	Not hardened.
21	88,000 and	22,000	176,000	
22	88,000 and	33,000	619,600	
23	88,000 and	33,000	2,135,670	Not broken after 35,800,000 flexures.
24	88,000 and	44,000		38,050,000 flexures.
25	88,000 and	44,000		36,000,000 flexures.
26	88,000 and	61,600		
27	77,000 and	11,000	286,100	
28	77,000 and	22,000	701,800	
29	77,000 and	27,500		36,600,000 flexures.
30	77,000 and	33,000		31,150,000 flexures.

that 121,000 lbs. will constitute the ultimate strength of the steel.

It is further *interpreted and assumed* that the *maximum* strains

99,000, 88,000, 77,000 lbs. can be carried for an infinitely long time if the *permanent* strains respectively are:

66,000, 44,000, 27,500 lbs. per square inch, so that the *variations* repeated *ad infinitum* would be:

33,000, 44,000, 49,500 lbs. per square inch.

It is seen that the variations decrease if the maxima increase, and the minima or permanent strains increase if the maxima increase. All strain is variable strain with no permanent strain for 55,000 lbs. (1), all is permanent strain with no variation for 121,000 lbs. per square inch (2).

The formula is made to *suit* these two final conditions by putting:

Maximum = 55,000 + Coefficients × unknown function.

The function must contain the minimum or permanent strain as factor, for then we get:

Maximum = 55,000 + Coefficients × (permanent or minimum strain) × other function; which by making the permanent or minimum strain = 0 gives:

Maximum = 55,000 + 0 = 55,000. This answers (1).

Again, put the maximum into the unknown function as divisor, and you get:

$$\text{Max.} = 55{,}000 + \text{coefficent} \times \frac{\text{permanent strain.}}{\text{maximum strain.}}$$

For the end relation (2) maximum = 121,000, all is permanent strain, and we get:

$$121{,}000 = 55{,}000 + \text{coefficient} \times \frac{121{,}000}{121{,}000},$$

or coefficient = 121,000 − 55,000 = 66,000 lbs.

The discovery is now made, and all we have to do is to manipulate the experiments so that it will answer them. For this purpose Herr Weyrauch has *assumed:*

					Ultimate strength.
Permanent strain...	0	27,500	44,000	66,000	121,000
Maximum strain for infinite durability by experiment....	55,000	77,000	88,000	99,000	121,000

which agree with the values:

By formula.....	55,000	78,200	88,000	99,000	121,000

We confess that we have not sufficient confidence in this test. Experiment 16 of the first series for strains of 66,000 lbs. caused rupture after 468,000 repetitions; hence it does not follow that 40,600,000 repetitions of strains of 55,000 lbs. constitute infinite durability.

Comparing series III. of Woehler's experiments, we find rupture to have taken place after 45,000,000 repetitions, while another bar, under precisely the same strain, broke after 4,163,000 repetitions.

Experiments 6 and 7 of Table III., on Bochum's steel, also give evidence that it is unsafe to consider even 57,000,000 repetitions as unlimited durability. A bar was strained to 33,000 lbs., when it broke after 57 millions of rotations; and yet another bar of the same material broke after 3½ million rotations, the strain being less and only 30,800 lbs. per sq. in.

The experiments 6, 7 and 8 of the second series quoted show that 35,000,000 impacts do not secure unlimited durability.

The variations from 77,000 to 132,000 gave rupture after 257,000 repetitions; the variations from 88,000 to 132,000 did not cause rupture after 35,600,000 repetitions; and yet the *easier* variations of 99,000 to 132,000 lbs. again gave rupture after 33½ million repetitions.

Again, it is doubtful whether a new set of experiments on the same class of *steel* bars would have been equally accommodating, and it is also unproved that the result holds good for other kinds of steel or for wrought iron.

We may assume that other "formulæ" give equally good results, and can be made to suit the experiments just as well, as far as they go.

That we know nothing definite about this subject is rather indicated by the different methods adopted by Gerber, Muller and Schæffer. The formula of Launhardt has the sole merit of being arranged to suit the beginning and the end of the series (the figures 55,000 and 121,000 again being only guessed at), and this can be done in many different ways by otherwise equally empirical expressions.

Herr Weyrauch censures the three gentlemen named for following too closely Woehler's results. This would rather constitute merit, in our eyes.

Woehler's experiments are not finished and not complete. They have the great merit of showing the right way, which is that of testing material in the *same* manner in which it is *used*. Woehler's experiments are of greater value still, because of the experimenter's cautious interpretation being free from new theories. He does not involve us in a multitude of new formulæ, and he especially points at the great amount of work still to be done. He says in his resume: "The strength of connections, as *riveting*, wedge connections, etc., as well as other forms, needs *special* investigations by *experiments*. The results of experiments on bars with sudden changes of sections and the influence observed of the pressure of axles on the wheel centers have proved sufficiently the necessity of such special experiments."

But instead of waiting for these very experiments, Herr Weyrauch has poured over us a flood of formulæ on riveting, all based on the empiricism of Launhardt.

There is another difficulty about this formula which is touched upon by Herr Weyrauch. What shall we do in regard to compression? Woehler's results in this regard only confirm what we knew, namely, that iron is more durable under pressure than under tension, proof of which is breakage of axles in their tensile parts. But more than that: the strength of long compression members cannot be measured by these experiments. Here we have undoubtedly to consider the *ultimate breaking strength* in some respect, for it does not follow that because a member stands with 10 tons per square inch it also must be safe with 10½ tons. Therefore the *maximum* strain is to be considered, whether caused by live or dead load. Herr Weyrauch has entirely neglected this subject.

Another reason why Launhardt's or any other similar formula is insufficient is this: According to Woehler's experiments, *rarely occurring* high strains may be admissible, whereas those strains that occur at every passage of a locomotive must be correspondingly lowered. But the Professors Launhardt and Weyrauch would consider only the *maximum strains* of each kind instead of the *average strains*. It is known that some theorists have taken special pleasure in supposing imaginary engines in all sorts of improbable positions on single and double-track bridges, and then have calculated or determined graphically the maximum maximorum. Such calculations should not be considered as of practical value. These questions also are touched upon by Herr Woehler, but have not been taken up by Prof. Weyrauch.

Of the other parts of the new book we have this to say:

Chapter V. treats of the ultimate tensile and compressive (crushing) strength of iron and steel bars, plates, wire, etc.; but this chapter is rather short and does not do justice to the importance of the subject. We find in a heap the average strength of Swedes, Lorraine, Low Moor, Borsig, etc., iron, without notice of their origin and without consideration of their qualities.

Also the other chapters on elastic limit, annealing and tempering, influence of form, amount of carbon, influence of temperature and valuation of material are somewhat too cursory and incomplete to give a clear representation of the various properties. However, there are many points touched upon to which attention is directed, all of which can be more fully studied in the original books from which the material is collected.

The inadmissibility of Launhardt's formula for riveting we have mentioned. We may add that if Herr Weyrauch had divested this chapter of algebraic expressions, and had carefully studied and explained the experimental results which are at our disposition, he probably could have given a very valuable addition to the literature on this subject, which is still remarkably poor, on account of deficient interpretation of otherwise valuable material. There are some excellent remarks contained in this part of the Professor's book which, though familiar to experts in detail construction, are not so generally known among railroad engineers as could be wished. Finally, Herr Weyrauch speaks of shape iron (Professor Dubois translates Facon-Eisen by "figured or ornamental iron") as of inferior tenacity because difficult to roll. We can however state that there is no reason why shape iron should be of inferior quality because of its manufacture. Where this is found it is more a proof of ignorance in roll-turning and working the material. Beams, channels, angles, etc., are rather of higher than inferior quality, other things being equal.

Weyrauch's book can not be recommended to those who know nothing about the quality of material and whose judgment in such matters does not stand above that of its author.

So far as the translation is concerned, Professor Dubois has taken great pains to come as near as possible to the author's original text. There are some slight oversights. Instead of "notches in the rail flanges" he translates "rail chairs." Instead of "resistance to crippling" of the compression flange of a girder, he translates "resistance of flexure." Instead of "vertical flange plate" of a girder he translates "stay plates" (*Steh-Bleche*).

Where Professor Weyrauch corrects his earlier views as to the saving of metal in continuous bridges, Mr. Dubois, who has himself some weakness as to this interesting theory, qualifies the author's sentence, "the saving of material by reason of continuity is thus less," etc., by adding "somewhat" before the word "less," which remark would have been better in a foot note.

Another translation of the same work comes from the house of D. Van Nostrand, who claims to have obtained the prior right to publish the work in the way agreed upon among American publishers of foreign books, by having first announced its intended publication. The Van Nostrand translation in some cases is more successful than the other in making the heavy sentences of the German author intelligible to the reader. It is a smaller volume than Professor Dubois' translation.